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CAUSALITY IN CONTEMPORARY PHYSICS (I) *

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1 *Introductory Remarks*

The number of conceivable, logically possible, physical worlds is infinite; the human imagination, however, is surprisingly poor in conceiving and working out new possibilities. The power of imagination is so limited by the intuitive conditions of gross perceptual experience that it can hardly by itself progress a step beyond them. It is only by aid of the strict discipline of more refined scientific experience that our thought can transcend its habitual channels. The most colourful fairyland of the Thousand and One Nights is created by a slight rearrangement of the familiar material of everyday life. And upon reflection, when one examines them with more precision, one finds the same to be true of the boldest and most profound philosophical systems: If for the poet it was creation by aid of intuitive pictures, so for the philosopher it is construction by more abstract yet still familiar concepts, from which by apparently more transparent principles of combination new structures are formed.

The physicist, too, at first proceeds in much the same way in the construction of hypotheses. This is particularly indicated by the tenacity of the belief, held for many centuries by the physicist, that to explain nature a copy of its processes in models perceptible to the senses is necessary. Thus, for instance, he repeatedly attributed characteristics of visible, tangible substances to the ether without the slightest reason for doing so. Only when observed facts either suggest or necessitate his use of the new systems of concepts does the physicist realise the new possibilities and break himself free from his former habits of thought;

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† I am much indebted to my colleague Professor Paul Feyerabend for advice concerning difficult passages.

but then he readily and with the greatest ease makes the jump to, say, Riemannian space or to Einsteinian time, to concepts so daring and profound that neither the imagination of the poet nor the intellect of the philosopher could have been able to anticipate them.

The turning point at which recent physics has arrived with respect to the question of *causality* could likewise not have been foreseen. Although there has been so much philosophising about determinism and indeterminism, about the content, validity, and mode of testing of the principle of causality, no one thought of precisely that possibility leading in quantum physics to the key that allows us a view of the real nature of the causal order. Only in retrospect do we realise how the new ideas differ from the old, and we are perhaps a little amazed that so far we have always missed the point. Now, however, after the significance of the concept of quantum theories has been demonstrated by the extraordinary results of its application, and we have had some years to accustom ourselves to the new ideas, it should not be premature to attempt to arrive at philosophical clarity as to the meaning and scope of the ideas that contemporary physics contributes to the problem of causality.

2 *Causality and the Principle of Causality*

The observation that philosophical meditations did not foresee possibilities that were found later on, because of their close adherence to existing ideas, is true also of the ideas I propounded more than ten years ago.[1] Still it is probably not useless to return to some points of those earlier considerations, since in this way the progress accomplished in the meantime becomes so much clearer.

First it is necessary to determine what the scientist actually means when he speaks of ' causality '. Where does he use this word? Obviously, wherever he supposes a ' dependence ' between certain events. (Nowadays it is self-evident that only events and not ' things ' come into question as elements of a causal relationship, since physics forms the four dimensional reality from events, and considers ' things ', three dimensional bodies, as mere abstractions.) But what does ' dependence ' mean? In science, in any case, it is always expressed by *a law*; causality is, accordingly, nothing but another word for the existence of a law. The content of the *principle* of causality then clearly lies in the assertion that *everything* in the world occurs according to laws; it is indifferent whether we affirm the validity of the principle of causality or of *determinism*. In order to formulate the principle of causality or the deterministic thesis, we must first have defined what is meant by a law of nature or by mutual ' dependence ' of natural events. For only when we know this are we able to understand the meaning of determinism, which states that *every* event is a member of a causal relation, that *every* process is wholly dependent upon other processes. (We shall not discuss whether the attempt to make a statement about ' all ' natural processes could lead to logical difficulties.)

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[1] *Naturwiss.*, 1920, 461 ff.

Thus in any event we distinguish the question of the meaning of the word ' causality ' or ' natural law ' from the question of the validity of the principle of causality or the law of causality, and we concern ourselves at the beginning with the first question only.

The distinction we thus make coincides with that made by H. Reichenbach at the beginning of his essay ' Die Kausalstruktur der Welt '.[1] He speaks there of the difference between two ' forms of the hypothesis of causality '. He calls the first the ' implication form '. It is given ' when physics establishes laws, that is, makes statements of the form: " if A then B ".' The second is ' the deterministic form of the causal hypothesis ' [2]; it is identical with determinism, which states that the course of the world as a whole ' remains unchangeable, that with a single cross section of the four dimensional world the past and future are fully determined '. It seems to me simpler and more to the point to characterise the difference involved as the difference between the concept of causality and the principle of causality.

The question then concerns the content of the concept of causality. When do we say that a process A ' determines ' another B, that B ' depends ' upon A, that B is related to A by *law*? What do the words ' if—then ', indicating causal relationship, mean in the statement ' if A then B '?

3 *Law and Order*

In the language of physics a natural process is represented as a sequence of values of definite physical magnitudes. We note already here that, of course, in a sequence only a finite number of values can be measured, that, therefore, experience affords only a discrete manifold of observed magnitudes, and further, that every value is conceived as subject to a certain inexactitude.

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[1] *Sitzungsber. bayer, Akad. Wis., Math.-physic.* Kl. 1925, 133

[2] ' Determinationsform der Kausalhypothese ' in the original (trans.)

Assuming that a large number of such observed magnitudes are given, we then ask quite generally: How does such a number of values have to be constituted so that we may say that it represents a *law-like* sequence, that there is a causal relationship between the observed magnitudes? We may, to begin with, presuppose that the data already possess a natural order, namely the spatio-temporal one; that is to say, each quantitative value relates to a definite position in space and time. It is of course true that only with the aid of causal considerations are we able to indicate the position of events in physical space-time, by passing from phenomenal space-time, which represents the natural order of our experiences, to the physical world. But this complication may be excluded in our considerations, which limit themselves entirely to the realm of the physical world. Furthermore, our considerations are based upon a most fundamental assumption which I here mention in passing only, since it has already been discussed in a previous work (loc. cit. p. 463). It is the hypothesis that in nature there are certain ' similarities ', in the sense that different realms of nature are *comparable* to one another, so that we may say for instance: ' the same ' *magnitude* that in this place has value f_1 has the value f_2 in another place. Comparability is then one of the presuppositions of measurability. It is not easy to give the real meaning of this assumption, but we need not concern ourselves with it since this last analysis is likewise irrelevant to our problem.

According to these observations, our problem regarding the content of the concept of causality reduces itself to this: what characteristic must the spatio-temporally ordered group of values have so that it may be regarded as an expression of a ' law of nature '? This characteristic can be nothing but again an *order*, and indeed, since events extensive in space and time are already *orderly*, it must be a kind of *intensive* order. This order must be of the *temporal* sort, for, as is well known, we do not speak of causality in reference to spatial order (popularly expressed by ' simultaneous ', coexistent events); the concept of activity finds no application there. Spatial regularities, if such there be, would be called ' coexistence laws '.

After limiting ourselves to the time dimension we must, I believe, now say: *Every* order of events in time, of whatever kind, is to be regarded as a causal relationship. Only complete chaos, complete irregularity, is to be designated as an acausal occurrence, as pure chance; every trace of an order would mean dependence, therefore causality. I believe that this use of the word ' causal ' is closer to its everyday sense than when confined, as seems to be done by many natural philosophers, to such an order as we could designate by ‘ complete causality ’—by which phrase it appears that something like ‘ complete determination ’ of the event in question is meant (of course, we can express ourselves here in inexact terms only). If we should restrict the word to complete causality, we run the danger of finding no use at all for it in nature, while *in some sense* we do regard the existence of causality as a fact of experience. And there would be even less reason to place the boundary between law and chance at some other point.

The only alternative that confronts us is thus: order or disorder? Causality and law are identical with order; irregularity and chance are identical with disorder.

The result up to the present therefore seems to be: we call a natural process, described by a group of values, causal or regular if the values show any temporal order whatever. This definition becomes meaningful only when we know what is to be understood by ‘ order ’, how it differs from chaos. A most puzzling problem!

4 *Attempts at Definition of Regularity*

It is certain that in our daily life as well as in science we differentiate rather clearly between order and disorder, regularity and irregularity. How are we to understand this? At first glance the answer does not seem very difficult. It appears that we need only make sure how physics actually represents laws of nature, in which form it describes the dependence of events. Now, this form is the mathematical *function.* The dependence of one event upon another is expressed by the fact that the values of a part of the magnitudes are represented as functions of the others. Every order of numbers is mathematically represented by a function; and so it appears as if the desired criterion of order, which differentiates it from disorder, is expressibility by a function.

But as soon as the idea of the identity of function and law is expressed, we see that it cannot possibly be correct. For, as is well-known, whatever be the distribution of given magnitudes, functions may *always* be found that represent just that distribution with any degree of accuracy; and this means that every possible distribution of magnitudes, every conceivable series of values is to be considered as an order. There would be no chaos.

Thus we do not in this way successfully distinguish causality from chance, order from disorder, or succeed in defining rule and law in this manner. As was shown also in our previous considerations, there seems to remain only the alternative of imposing certain requirements on the functions that describe the observed series of values, and by means of them to determine the concept of order. We should then have to say: if the functions that describe the distribution of values of the magnitudes have such and such a definite structure, the represented sequence is in accordance with law, otherwise it is unordered.

Thereby we find ourselves in a rather hopeless situation, for it is clear that in this way arbitrariness is given free rein, and a distinction between law and chance resting upon such an arbitrary basis could never be satisfactory. It could be so only if a fundamental and sharp distinction in the structure of the functions could be ascertained, which at the same time possessed such definite empirical possibilities of application that everyone would immediately recognise them as the correct formulation of the concepts of regularity and irregularity as they are applied in science.

Here there appear simultaneously two ways, both of which men have tried to adopt. The first was already used by Maxwell to define causality. It consists of the following stipulation: the space and time co-ordinates are not to appear explicitly in the equations that describe the sequence in question. This requirement is equivalent to the notion popularly expressed in the phrase: similar causes, similar effects. In fact it means that a process that takes place anywhere and any time in a definite manner will take place in exactly the same manner in every other place and time, under the same circumstances. In other words, the rule states the *universal validity* of the relationship represented. Universal validity, however, is, as has been generally acknowledged, exactly that which in laws of nature has been designated by the ambiguous term ‘ necessity ’, so that it appears as if the essential nature of the causal relation had been correctly hit upon by this stipulation.

Concerning Maxwell's definition of law, which I myself previously defended (in the passage cited), we may say the following:

The concept of law in physics is undoubtedly such that this requirement is always fulfilled. Actually no investigator thinks of formulating laws of nature that refer explicitly to definite positions and moments in the universe. If space and time occurred explicitly in the physical equations, they would have quite a different significance from the one they actually have in our world. The relativity of space and time, fundamental to our world-view, would be denied, and time and space could no longer assume the peculiar role of ‘ forms ’ of occurrence which they have in our cosmos. We should therefore be free to maintain the Maxwellian condition of causality—would it, however, be a necessary condition? We shall hardly be allowed to say that, for surely a world is conceivable in which all events would have to be expressed in formulas in which space and time appear explicitly, without our denying that these formulas represent true laws and that this world is completely orderly. So far as I can see, it would be conceivable, for instance, that uniform measurements of the elementary quantum of electricity (electric charge) would give values for these magnitudes that would fluctuate about 5 per cent in seven hours and then again in seven hours, and then ten hours, without our being able to find the slightest ' reason ' for it. And besides that, perhaps still another variation might appear for which we would make an absolute change of position of the earth in space responsible. In this case then the Maxwellian condition would not be fulfilled, but we would surely not find the world to be disorderly and we would formulate its regularity and be able to make predictions by means of it. We shall therefore be inclined to the view that the Maxwellian definition is too limited, and we shall ask ourselves what the criterion of law would be in the hypothetical case we have discussed.

Now, the decisive factor of the hypothetical case seems to be that we could so easily consider the influence of space and time, that they enter into the formulas in such a *simple* manner. If, in our example, the electric charge were to behave differently every week and every hour, or form a completely ' irregular curve ', we could of course afterwards represent its dependence upon time by a function, but this function would be very complicated. We would then say that no law exists but that the variations of magnitudes are governed by ' chance '. We do not have to invent cases of this kind since, as is well known, the new physics accepts them as commonplace. The discontinuous events in the atom that Bohr's theory interpreted as jumps of an electron from one orbit into another are regarded as purely accidental, as ' acausal ', although we may subsequently think of their occurrence as a function of time. But this function would be very complicated, not periodic, not readily grasped, and it is for this reason only that we say that no regularity exists. But as soon as any simple statement regarding the jumps is formulable—if, for instance, the time intervals become increasingly larger—this would at once appear to us as a regularity, even though time would explicitly enter into the formula.

Accordingly, it appears as if we speak of order, law, causality when the course of events is described by functions of *simple* form; while complexity of the formula is the criterion for disorder, lawlessness, chance. And so one very easily arrives at the point of defining causality by the *simplicity* of the descriptive functions. Simplicity is, however, a half pragmatic, half aesthetic concept. We may therefore call this definition aesthetic. Also, without being able to state what is here meant by ' simplicity ', we must yet affirm the fact that every investigator who has succeeded in representing a series of observations in a simple formula (for instance, linear, quadratic, exponential function) is quite sure of having discovered a *law*, and so the aesthetic definition, as well as the Maxwellian one, obviously discloses a characteristic of causality that is considered a decisive criterion. Which of the two attempts at formulating the concept of law shall we accept? Or shall we formulate a new definition by combining both?

5 *Inadequacy of the attempts at definition*

To sum up: the Maxwellian definition has *in its favour* the fact that all known laws of nature satisfy it, and that it may be considered to be an adequate expression of the proposition, ' similar causes, similar effects '. *Against* this definition is the fact that cases are *conceivable* in which we should certainly acknowledge regularity without the fulfilment of the criterion.

The ' aesthetic ' definition has *in its favour* that it is also applicable to the above considered cases, to which the other one is not, and that also undoubtedly in the prosecution of science ' simplicity ' of functions is used as a criterion of order and law. *Against* it, however, is the fact that simplicity is clearly a relative and indefinite concept, so that a strict definition of causality is not obtained, and law and chance cannot be sufficiently distinguished. It might indeed be possible that we have to take this last idea into account, and that a ' law of nature ' is actually not something so precisely conceivable as one might at first think; however, such a point of view will be accepted only when one is sure that no other possibility remains.

It is certain that the concept of simplicity can only be fixed by a convention that must always remain arbitrary. We should probably be inclined to consider a function of the first degree simpler than a function of the second degree; however, even the latter undoubtedly represents an unexceptionable law when it describes the data of observation with great accuracy. The Newtonian formula of gravitation, in which the square of the distance occurs, is generally still regarded as a paradigm case of a simple natural law. One may, for example, agree further that of all continuous curves that pass through a given number of points with sufficient proximity, we may consider as simplest the one that everywhere on the average has the greatest radius of curvature. (There is an unpublished work on this by Marcel Natkin.) However, such artifices seem unnatural, and the fact alone that there are *degrees* of simplicity makes the definition of causality based upon it unsatisfactory.

This state of affairs is made even worse by the fact that, as we know, it is not at all a matter of the simplicity of an isolated law, but of the simplicity of the system of all natural laws. And so, for instance, the true equation of the law of gases has by no means the simple form given to it by Boyle-Mariotte; yet we know that its complicated form may be explained by a particularly simple set of elementary laws. In principle it should be much more difficult to find rules for the simplicity of a *system* of formulas. They would always remain provisional, so that apparent order could with progressive knowledge turn out to be disorder.

And so neither the Maxwellian nor the aesthetic criterion seems to give a really satisfactory answer to the question as to what causality actually is. The first seems too narrow, the second too vague. No progress in principle is made by a combination of both attempts, and one may readily see that the shortcomings cannot be removed by improvements along the same lines. The shortcomings observed are clearly of a fundamental nature, and that gives us the idea of revising the present point of departure and of considering whether we are in general on the right path.

6 *Prediction as Criterion of Causality*

Until now we have assumed that a definite distribution of values is given, and asked: when does it represent a regular and when a chance sequence? It may be that this question cannot be answered at all by mere consideration of the distribution of values, but that it is necessary to go beyond this domain.

Let us for a moment consider the consequences that the statements made about the *concept* of causality have for the *principle* of causality. We imagine that for as many internal and boundary points of a physical system as possible we attempt to determine the value of the state variables by precise observation. Now one is in the habit of saying that the principle of causality is valid if from the state of the system during a very short time and from the boundary conditions all other states of the system may be deduced. Such a deduction is, however, possible *under all circumstances*, for according to what has been said, functions may always be found that represent all observed values with any desired accuracy. And as soon as we have such functions we may by means of them compute all states *already observed*, whether earlier or later, from any state of the system. For functions have been chosen in such a way that they represent everything observed in the system. In other words: the principle of causality would be satisfied *under all circumstances*. A statement, however, that is applicable to any system whatever, no matter what its properties, says nothing at all about this system, is an *empty* statement, a mere tautology, and it is futile to construct it. Hence if the causal law is actually to mean something, if it has content, the formulation from which we began must be false, for the law has turned out to be tautological. If, however, we make the qualification that the equations used are not to contain the space and time co-ordinates explicitly, or that they are to be very ' simple ', the principle acquires, to be sure, a real content; but, in the first case the reflection is valid that we have formulated too limited a concept of causality, and in the second the sole characteristic would be that the computation would be easier. However, we should certainly not want to formulate the difference between chaos and order in such manner that we say the first is comprehensible only to an excellent mathematician, the second to an average one.

We must therefore begin anew and attempt to formulate the meaning of the causal law differently. Our error until now was that we did not conform with enough precision to the actual procedure by which, in science, one actually tests whether processes are or are not dependent upon one another, whether or not a law, a causal sequence, exists. Until now we only investigated how a law is *constructed*. To learn its real meaning, however, one must observe how it is *tested*. It is always the case that the significance of a statement is revealed only by the manner of its verification. How then is the test made?

After we have succeeded in finding a function that satisfactorily connects a group of observational data, we are by no means satisfied, even when the function found has a very simple structure, for now comes the main thing, which our considerations hitherto have not touched: We observe whether the formula obtained also represents correctly those observations that *were not used* in achieving the formula. For the physicist, as an investigator of reality, the only important, decisive, and essential thing is that equations derived from certain data be applicable to other, *new* data. Only when this is true does he consider his formula to be a law. In other words, the true criterion of law, the essential sign of causality, is the *success of prediction.*

By success of prediction is to be understood, according to what has been said, nothing but the confirmation of a formula for such data as have not been used in its construction. Whether these data have already been observed or are only subsequently determined is in this connection of no consequence whatever. This observation is of great importance: past and future data are altogether on the same footing in this respect, the future is not of special significance; the criterion of causality is not confirmation in the future but confirmation in general.

It is self-evident that the test of a law can occur only *after* its formulation, but this gives no special distinction to the future. What is essential is that it is indifferent whether the verifying data are in the past or the future; it is incidental when they become known or are used for verification. The confirmation remains the same whether the data were known before the formulation of a theory, as in the case of the anomaly of Mercury's movement, or whether it was prophesied by the theory, as in the case of the red-shift of the spectral lines. Only for the *application* of science, for technique, is it of fundamental importance that natural laws allow prediction of something new, observed by no one as yet. And so earlier philosophers, Bacon, Hume, Comte, have long known that knowledge of reality coincides with the possibility of prediction. Thus fundamentally they correctly formulated the essence of causality.

7 *Elucidation of the Result*

If we accept the success of prediction as the true criterion of a causal relationship—and, with an important limitation to be mentioned presently, we shall have to do so—we thereby admit as well that the previous attempts at definition no longer enter into consideration. In fact, if we can really predict new observations, it does not matter how the formulas that enabled us to do so were constructed, whether they seem simple or complicated, whether time and space enter explicitly or not. As soon as someone can calculate the new observation data from the old, we shall admit that he has grasped the law governing the processes; prediction is therefore a sufficient characteristic of causality.

We easily realise that confirmation is also a necessary characteristic, and that the Maxwellian and aesthetic criteria do not suffice, when we imagine we have found a formula of great simplicity that describes a definite natural process with great precision but at once ceases to work when applied to the further phases of the process, to new observations. Obviously we should then say that the distribution of magnitudes occurring once has simulated a dependence of natural events which in reality does not exist, that it was a matter of mere chance the particular sequence could be described by simple formulas. That there was no natural law is proven by the fact that our formula can stand no test, for in the attempt to repeat the observations the sequence occurs quite differently; the formula is no longer applicable. A second alternative seems, of course, to be that one may say the law was valid at the time of the observation but no longer holds. It is clear, however, that this is only another way of expressing the absence of law, the universality of the law being denied. The 'regularity' observed for the single sequence was not true regularity, but merely chance. The confirmation of prediction is therefore the *only* criterion of causality. Through it alone does reality speak to us; the construction of laws and formulas is simply the work of man.

Here I must include two observations that go together and are of basic importance. First, I said previously that we may recognise the 'verification' of a regularity as the adequate characteristic of causality only subject to a limitation. This limitation consists in the fact that the confirmation of a prediction never actually *proves* the existence of causality but always only makes it *probable*. Further observations may indeed show the supposed law to be always incorrect, and then we should have to say that 'it expressed the sequence only by chance'. A final verification is therefore, so to say, impossible in principle. We deduce therefrom that a causal statement logically does not at all have the characteristic of a *proposition*, for a genuine proposition must in the end allow itself to be verified. We shall return to this shortly without, however, being able to explain this apparent paradox fully here, where we are not concerned with logic.

The second observation concerns the fact that between the criterion of confirmation and the two rejected attempts at definition a remarkable relationship exists nevertheless. It lies simply in the fact that *actually* the different characteristics go hand in hand. We certainly expect with great assurance that precisely those formulas satisfying the Maxwellian criterion and distinguished by aesthetic simplicity will be confirmed, and that the propositions made by their help will be true. And even if we should sometimes be disappointed in this expectation, the fact remains that the laws that have really proved to be valid were always of a profound simplicity, and always fulfilled the Maxwellian definition. But what the significance of this ' simplicity ' is, is difficult to say, and much erroneous thinking has been done in this connection; we do not wish to put too much stress upon it. It is certain that we may imagine much ' simpler ' worlds than our own. There is also a ' simplicity ' that is merely a matter of *representation*, that is, pertains to symbolism by means of which we express facts. Its consideration leads to the question of ' conventionalism ' and does not interest us in this connection.

At any rate we see that if a formula corresponds to both of the earlier and inadequate criteria we consider it probable that it is really the expression of a law, of an actually existing order, that it will therefore be *confirmed*. If it has been confirmed, we think it again probable that it will continue to be so. (And indeed, it is understood, without bringing in new hypotheses, for in general physical laws are so constructed that they may always be maintained by new hypotheses brought in *ad hoc*. But if these become too complicated, one says that the law nevertheless does not hold, the right order has not been found.) The word probability, which we use here, moreover designates something completely different from the concept treated in the calculus of probability and occurring in statistical physics.[1]

For the sake of logical clarity (for philosophers this is the prime concern) it is of greatest importance to realise the situation precisely. We have seen that, basically, causality is not at all definable in the sense that for an *already given* sequence one could answer the question: Was it causal or not? Only in reference to the *single case*, to the single verification, can one say: It behaves as causality demands. For advancement in knowledge of nature (and this is the main concern of the physicist) this is fortunately quite sufficient. If a few verifications—under some circumstances only one—are successful, we build practically on the verified law, with the unqualified reliance with which we trust our life to a motor constructed according to natural laws.

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[1] Cf. F. Waismann, ' Logische Analyse des Wahrscheinlichkeitsbegriffs ', *Erkenntnis* I, 238, with whose analysis I find myself in principle in full agreement.

It has indeed been frequently observed that one cannot actually speak of an absolute verification of a law, since we always, so to say, silently reserve the right to modify it on the basis of further experience. If I may in passing add a few words about the logical situation, the circumstance mentioned means that basically a natural law does not have the logical character of a ‘ proposition ’ but represents ‘ a direction for the formulation of propositions ’. (I owe these ideas and terms to Ludwig Wittgenstein.) We have already indicated this, above, regarding causal statements, and actually a causal statement is identical with a law. The statement, ‘ The principle of energy holds ’, for instance, says no more nor less about nature than the principle of energy itself says. As is well known, only the individual propositions derived from a natural law are testable, and these always have the form: ‘ Under such and such circumstances this indicator will point to that mark on the scale ’, ‘ Under such and such circumstances there occurs a darkening on this point of the photographic plate ’, and the like. The verifiable propositions are of this nature and of this nature is every verification.

Verification in general, the success of a prediction, confirmation in experience, is therefore the criterion of causality, simply; and indeed in the practical sense in which alone we may speak of the test of a law. In this sense, however, the question regarding the existence of causality *is* testable. That confirmation in experience, the success of a prediction, is something *final*, not subject to further analysis, cannot be overemphasised. No number of propositions can state when it must occur, but we must simply await whether it occur or not.

8 *Causality and Quantum Theory*

In the previous considerations nothing was said except what, in my opinion, may be read out of the procedure of the scientist. No concept of causality was constructed; only the rôle it actually plays in physics was determined. Now the attitude of most physicists towards certain results of quantum theory shows that they see the essence of causality just where the foregoing considerations also found it, namely, in the possibility of *prediction*. When the physicists say that complete validity of the causal principle is not compatible with the quantum theory, the basis, indeed the *meaning*, of this assertion, lies simply in the fact that the theory makes precise *predictions* impossible. We must try to make this really clear to ourselves.

In contemporary physics it is possible to say, in a manner of speaking, that, with certain limitations to be mentioned, each physical system is to be considered as a system of protons and electrons, and that its state is completely determined by the position and momentum of its particles being known at every moment. Now, as is well known, a certain formula is derived from the quantum theory, the so-called ' uncertainty principle ' of Heisenberg, which teaches that it is impossible to indicate for a particle *both* determinants, place *and* velocity, with any desired precision, and that the more precise the value of one co-ordinate the greater inexactness we must expect for the other. If we know say that the place co-ordinate lies within a small interval Δp, the velocity co-ordinate q may be indicated only with such precision that its value remains undetermined in the interval Δq, and indeed so that the product $\Delta p \, \Delta q$ is of the order of magnitude of Planck's quantum effect h. In principle then the one co-ordinate could be determined with any degree of precision, but absolutely precise observation of it will have as a consequence that we can say *nothing more* about the other co-ordinate.

This principle of indeterminacy has been so frequently illustrated, even in popular form, that we need not describe the situation any more closely; our task must be to understand exactly its real *meaning*. When we ask for the meaning of a statement this always means (not only in physics): by which particular experiences do we test its truth? When thus, for example, we conceive the place of an electron to be determined by observation with an inexactness Δp, what does it mean when I say, for instance, the direction of the velocity of this electron may be indicated only with an inexactness $\Delta \theta$? How do I determine whether this statement is true or false?

Now, that a particle has gone in a definite direction may be tested only by its arrival at a definite point. To give the velocity of a particle *signifies* absolutely nothing more than to predict that in a certain time it will arrive at a certain point. ' The inexactness of direction amounts to $\Delta \theta$ ' means: in a certain experiment I shall find the electron within the angle $\Delta \theta$; however, I do not know exactly *where* therein. And if I repeat ' the same ' experiment I shall find the electron at various points within the angle, and I never know *beforehand* at which point in it. If the position of the particle is observed with absolute precision the result would be that in principle we could not know at all in which direction the electron would be found after a short time. Only further observation could subsequently tell us this, and with very frequent repetition of ' the same ' experiment it must appear that on the average no one direction predominates.

The fact that both position and velocity of an electron cannot be precisely measured is usually interpreted as saying: it would be impossible to describe fully the state of a system at a definite point in time, and therefore the principle of causality becomes inapplicable. Since the principle asserts that the future states of the system are determined by its initial state, since, thus, it presupposes that the initial state may be described in principle exactly, the principle of causality collapses, for this presupposition has not been fulfilled. I should not like to call this idea false, but it seems useless to me, because it does not express the essential point clearly. What is essential is that one realise that the indeterminacy that the Heisenberg-relation expresses is in truth an indeterminacy of *prediction.*

In principle nothing interferes (this is also emphasised by Eddington in a similar thought context) with our determining the position of an electron twice at any two closely adjacent points in time, and with our considering these measurements equivalent to position and velocity measurements. But the vital point is that with data about a state obtained in such a manner we are never in a position to predict a future state with precision. If, that is, we should define the velocity of the electron in the usual manner (distance divided by time) by means of the observed places and times, the velocity would nevertheless be different in the next moment, for, as we know, it must be assumed that its course is disturbed by observation in a quite uncontrollable manner. This alone is the true significance of the statement that a momentary state is not precisely determinable; that is, the impossibility of prediction alone is the actual reason why the physicist deems necessary the denial of causality.

There is no doubt, therefore, that quantum physics finds the criterion of causality precisely where we too have discovered it, and speaks of the failure of the principle of causality only because it has become impossible to make predictions with any desired degree of accuracy. I cite M. Born, *Naturwiss.* 17 (1929):

> The impossibility of measuring exactly all the data of a state prevents the predetermination of its further course of development. Because of this the principle of causality in its usual formulation loses all significance. For when it is impossible in principle to know all the conditions (causes) of an event, it is empty talk to say that every event has a cause.

Causality as such, the existence of laws, is however not denied. There are still valid predictions, but they do not consist in the expression of exact magnitude values, but are of the form: the magnitude X will lie in the interval between a and Δa.

What is new in the contribution of the most recent physics to the problem of causality does not consist in the fact that the validity of the causal principle is contested at all, nor that, say, the micro-structure of nature is described by statistical rather than causal regularities, nor in the fact that the realisation of the merely probable validity of natural laws has displaced belief in their absolute validity. All these ideas have, in part, long since been expressed. The novelty rather consists in the hitherto unsuspected discovery that through natural laws themselves a limit is set in principle to precision in predictions. That is something quite different from the rather obvious idea that actually and practically there is a limit to precision in observations and that the assumption of absolutely precise natural laws is in every case unnecessary if one wants to give an account of every experience. Previously it must always have seemed as if the question of determinism had to remain undecided in principle. The kind of decision now available, namely, by means of a natural law itself (the Heisenberg relation), was not foreseen. In any case, one who today speaks of a decidability and holds the question to be answered unfavourably for determinism must assume that law of nature as actually existing and raised beyond all doubt. That we are absolutely sure of this, or ever could be, a careful investigator will hesitate to state. But the principle of indeterminacy is an integral part of the structure of the quantum theory, and we must trust its correctness so long as new experiments and new observations do not force us to revise the quantum theory. (In fact it is daily better confirmed.) But to have shown that a theory of such structure is at all possible in the description of nature is in itself a great accomplishment of modern physics. It signifies an important philosophical clarification of the basic concepts of natural science. The progress in principle is clear. One may now speak of an empirical test of the principle of causality *in the same sense* as of the test of some special law of nature. And that we may in some sense justifiably speak of it is proven simply by the existence of science.

(*To be concluded*)

DISCOVERING THE POSITRON (I)*

Norwood Russell Hanson

'On August 2, 1932, Dr Carl D. Anderson discovered the positive electron.'

This remark is often repeated in textbooks and manuals on modern physics. It is singled out here for several reasons: (1) Its innocent, matter-of-fact tone conceals one of the most intricate and interesting chapters in the history of scientific discovery; (2) it encourages an analogy between discoveries within microphysics, and quite non-comparable discoveries within, e.g., natural history; and (3) the actual discovery of the positron, in all its dimensions, constitutes an instructive example of the interplay of theory and experiment within physical science, one of the best a historian, or logician, of science could ever hope for.

The positron packet can be dipped into in three different ways. The physicist reaches into this complex of concepts to facilitate his thinking within e.g. quantum field theory, or the experimental study of cosmic rays. The historian of physics attends to the exciting interplay of ideas within the microphysics of the 1920s—the better to perceive the initial resistance to the positron-idea, as well as to trace the theoretical evolution which resulted in the claim that positive electrons exist. The logician will focus upon the internal structure of the arguments and concepts actually employed, and still being employed, by those physicists who have played major rôles in this story. In the history of science the positron discovery constitutes at once one of the richest, and yet one of the most neglected, areas of inquiry.

The discovery of the positive electron was a discovery of three different particles. I choose to put the matter this way, rather than to remark three different discoveries of the same particle, because the conceptual backgrounds within which the work of Dirac, Anderson, and Blackett took place were so disparate as to leave it unclear until almost 1934 whether their findings had anything in common. Perhaps this is not so dramatically apparent in the case of Anderson and Blackett, since the latter knew immediately that 'his' particle was the same as Anderson's. Dirac's work is quite distinct, however.

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* Received 10. ix. 60. During the last six years this research has been helped by many. The Nuffield Foundation, The Minnesota Center for Philosophy of Science and Indiana University were especially encouraging. Moreover, the principals within this intricate drama of ideas are primarily responsible for whatever merit there may be in what follows. Professors Anderson, Dirac, Blackett, Oppenheimer, Mott, Bethe, Bohr, Heisenberg, Lamb, Skobeltzyn, Rosenfeld, Wilkinson, and Joliot-Curie helped with references, interpretations, and recollections. Professors Ryle, Feigl, Maxwell, Sellars, and Toulmin made valuable suggestions. I regret only that their efforts cannot guarantee that the sequel is free from errors and imperfections. These are, of course, solely the author's responsibility.

Indeed, a contemporary reflection of this is found in the ways in which scientists now describe the discovery. Thus Millikan, writing in 1935, says '. . . the discovery of the positive electron . . . was made without the guidance of any theory whatever, as was also the discovery of the frequent occurrence of positive-negative pairs of tracks . . .'.[1] Professor Blackett describes the discovery in much the same way, as do also Dr Oppenheimer and Anderson himself.[2] On the other hand, Professor Hans Bethe feels the discovery to have been primarily a theoretical one, on the grounds that positron tracks had been photographed before 1932. However, lacking a theory in virtue of which such tracks might be made intelligible, physicists failed to identify them. Indeed, Professor Konopinski avers that 'every theoretical physicist knows that Dirac discovered the positron'. Thus, even today, these historically different approaches to the particle remain unresolved.

Accordingly, our study will be designed as a triptych; we shall discuss (1) The Anderson Particle, (2) The Dirac Particle, and (3) The Blackett-Occhialini Particle.

Even before considering Anderson's research, some account of the genesis of the present essay may be in order. Professor P. A. M. Dirac once spoke to me of a lecture given at the Cavendish by D. Skobeltzyn, 'sometime in 1926 or 1927'. Dirac recalls the description by someone, perhaps Skobeltzyn, perhaps someone in the audience, of an experimental setup within which Skobeltzyn was bombarding a metal target. One of the curiosities reportedly mentioned by Skobeltzyn was that several particles which were certainly electrons were seen to 'fall back into the source'; this, despite the fact that most of the electrons moved in the way usual for this experiment, *away* from the source. Professor Dirac feels that what he remembers Skobeltzyn as having then described could only have been positive electrons, and he suggests that the Russian might very well then have made the discovery.

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[1] R. A. Millikan, *Electron* (Cambridge, 1935), p. 339. Compare Neddermeyer, Anderson's collaborator in 1932 and 1937, writing in the *Physical Review*, March 15, 1961 : 'However, it is not to discredit theory or theoreticians to point out that the muon, like the positron, was a purely experimental discovery in the sense that it was made entirely independently of any theoretical considerations of what particles should and should not exist. . . .' (p. 1814).

[2] These sentiments were disclosed to the author during conversations and correspondence with the physicists mentioned.

My reaction was that it would have been an immensely difficult feat of imagination for Skobeltzyn, or anyone else, to have made this discovery in 1926, given the theoretical conceptions and experimental facilities then available. An exploration of Skobeltzyn's published work entirely failed to reveal any photograph or even a passing remark, relevant to Dirac's narrative. However, a related photograph did turn up in *Zeitschrift für Physik*, 1927, **43**, 362. Schematically-reproduced below is part of the lower left hand photograph on that page:

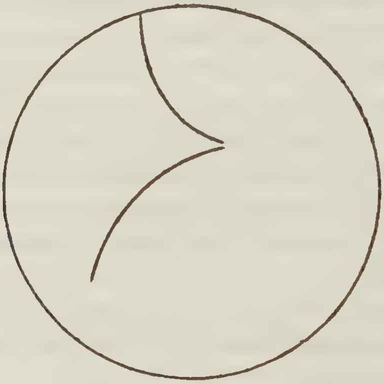

Fig. 1.

Let me stress that this configuration is probably not what it appears to be. None the less, the presence of geometrically-associated tracks, of the sort one expects with positron-negatron pairs, will be evident. As a check on my own reactions this photograph was shown to many experimentalists. Without exception, all granted that this might well be a pair; some thought it a very good example of one. No stereoscope was used in this survey, however.

Professor Skobeltzyn, in two communications of 1956 and 1960, offers a different interpretation of this photograph. He takes it to be a chance overlapping of two different tracks, one of a Compton recoil-electron, and the other of a spurious track emerging from the chamber wall. Without doubt, Skobeltzyn is correct, but since the original plates are destroyed, the use of a Pulfrich Stereocomparator is not now possible. The foregoing plate is to be understood more as a trigger for my further inquiries concerning pre-Anderson positrons, than, as had until recently been felt, an actual disclosure of such a particle. For, whether justified by the event or not, the present author was encouraged by this photograph to seek further pre-1930 evidence of positive electrons. It can be confidently supposed that the tell-tale tracks were ' encountered ' by researchers like Orban, Rochester, Terroux, Feather, and certainly by Meitner, and the Joliot-Curies. Indeed, in pointing up the mistake in Dirac's reports and recollections concerning 1926-27, Skobeltzyn refers me to the work of Joliot and Curie of early 1932 in which mention is made of ' backward electrons '.[1]

Skobeltzyn suggests that Dirac may have confused this work of Joliot and Curie with his memory of the 1926-27 lecture at the Cavendish. Perhaps. But Blackett, Meitner, and others certainly encountered electrons, before Anderson's discovery, which they described as ' falling back into the source ', ' curving the wrong way ', ' coming up from the floor ', or ' moving backwards '. Skobeltzyn disagrees with my contention that several microphysicists saw, but did not observe, positron tracks prior to 1930. He writes:

> None of them published their results earlier than in 1930. Besides, Williams and Terroux used in their work RaE, a source which does not emit γ-rays. Meitner-Filipp published their observation in 1933 and certainly did not use the Wilson chamber combined with a magnetic field until 1932. Ellis never worked with a Wilson chamber. Joliot-Curie began the observation with a Wilson chamber placed in a magnetic field only in 1932 (certainly not earlier than the end of 1931, i.e., definitely after my departure from Paris in August 1931). D. S. [Letter to the author, 22nd October 1960.]

Professor Skobeltzyn's views must, of course, be given the greatest weight. Against this, however, must be balanced the reports of Dirac, Blackett, and Bethe, all of whom have expressed to me their conviction that tracks were encountered, but not identified, long before Anderson's discovery. Since this essay attempts to provide an historical and conceptual context for this reported conviction, I have placed Skobeltzyn's views alongside those of other physicists to give the reader the same data that the author has.

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[1] ' . . . il nous a paru que le parcours des rayons H projetés par les neutrons émis vers l'arrière, par rapport à la direction des particules α, est plus grand que celui qu'on peut prévoir par le calcul . . . Un rayonnement très pénétrant s'absorbant par projection de noyaux et émis dissymétriquement par rapport à la direction des particules α incidents . . . '. (I. Curie and F. Joliot, *Comptes Rendues de l'Académie des Sciences*, **194,** 1232; 11th April 1932.)

In this connection, the following is relevant:

> I heard about ' electrons falling back into the source ' at a colloquium at the Cavendish. . . . Someone was reporting on the work of Skobeltzyn, the Russian physicist . . . I would suggest you look up *Science Abstracts* for the years 1925-30 . . . [Dirac, letter to the author, 12th August 1955.]
> . . . all sorts of people saw positrons well before Anderson. Blackett certainly did and was always puzzled by the number of electrons that came upwards from the floor . . . [D. H. Wilkinson, letter to the author, 12th December 1955.]
> . . . Nobody took ' experimental ' positive electrons before 1933 seriously, nor did Dirac make the connection in 1931. The theoretical development was certainly taken much more seriously . . . [H. Bethe, communication to the author, early 1956.]

Whenever seen, such tracks were discounted as ' spurious ', or as ' dirt effects '. Certainly, no experimental physicist before late 1932 made any such track his prime object of study. Part of the function of our inquiry will be to understand why this is so, why such tracks were always overlooked, underevaluated, or explained away. Why did the idea of a positively-charged electron seem so completely untenable?

1 *The Anderson Particle*

Had anyone ever seriously suggested that Professor Anderson's discovery of the positive electron was contingent upon the prior publication of Dirac's positron theory in 1931 [I once suggested this], Anderson could easily refute him. Neither Dirac's book *Quantum Mechanics* (1930), nor his great paper on the spinning electron (1928) [1] had even been read by Anderson before 1932. Indeed, he claims not to have fully understood either of these works even when he read them later, having been encouraged to do so by the publication of Blackett-Occhialini in 1933. Hence it could hardly be claimed that Anderson's discovery of the positron on 2nd August 1932, was the result of Dirac's theoretical breakthrough. No, Anderson's discovery was of a different kind.[2]

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[1] *The Principles of Quantum Mechanics*, Oxford, 1930; ' The Quantum Theory of the Electron ', *Proc. Roy. Soc.*, 1928, **117**, 610; **118**, 351

[2] Dr Oppenheimer writes: ' . . . I remember talking with Anderson about the possible relevance of his tracks and the Dirac theory. His attitude was that he did not understand the theory, and could not take a responsible view of it: and that therefore his discovery had to be soundly based on experimental material . . . ' [Letter to the author, 12th August 1960.]

The celebrated photograph sketched below is that for which Carl Anderson is honoured. But the honours accrue not simply to his camera technique and experimental ingenuity.[1] For here was a cloud chamber track which might also have been rejected as ' spurious ', or even as a ' dirt effect '. Soon we will examine the reasons for which Anderson's predecessors and contemporaries were so reluctant to admit a positive electron. Theory was against it. Observation was against it. The giants of physics, Bohr and Rutherford, were against it. But Anderson had the courage, and the cool logic, to be for it. Although he knew of no established theoretical route along which to infer the existence of a positron electron, Anderson's honest and rigorous interpretation of the photograph schematically-reproduced above forced him to hack out his own argument *ab initio*:

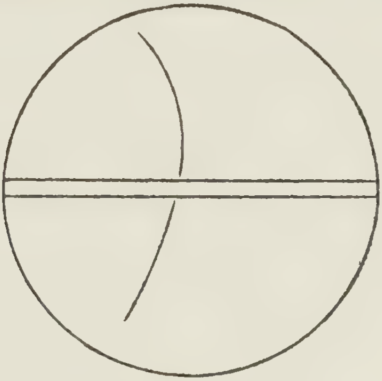

Fig. 2.

> . . . the track . . . cannot possibly have a mass . . . of a proton . . . [. . . the length . . . is . . . ten times greater than . . . a proton path of this curvature] . . . assume that at exactly the same instant . . . two independent electrons happened to produce two tracks . . . to give the impression of a single particle. . . . This . . . was dismissed on a probability basis . . . we also discarded . . . the assumption of an electron of 20 million volts entering the lead on one side and coming out with . . . 60 million volts. Other photographs were obtained which could be interpreted logically only on a positive-electron basis . . . (op. cit. p. 491).

In short, Anderson reasoned that the particle was coming up from below: it lost energy in the plate and curved more markedly towards the negative pole of the transverse magnetic field surrounding the chamber. This established that the particle, already identified as an electron by its range, was positively-charged.

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[1] C. D. Anderson, ' The Positive Electron ', *Physical Review*, 1933, **43**, 492

In his distinguished cosmic ray work of early 1931 Professor R. A. Millikan had already obtained two remarkable photographs, sketched below.[1] These record incoming cosmic rays encountering an atomic nucleus, out of which are projected a positive and a negative particle.

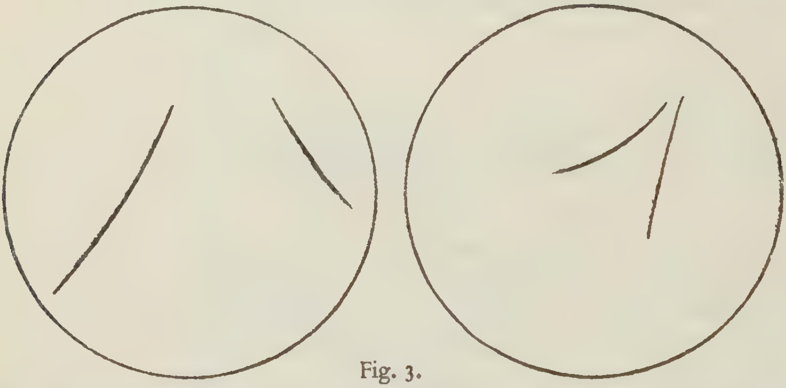

Fig. 3.

Millikan's photographs established that the nucleus is important in cosmic-ray absorption; moreover, both positives and negatives can be ejected from a nucleus hit by a cosmic ray. In 1919 Rutherford had shown that α-particles could dislodge protons from the nuclei of light atoms. This was the nearest analogy to Millikan's observations. But cosmic rays are not streams of α-particles. So here was a new phenomenon.

In 1931, during lectures at Cambridge and Paris, Millikan interpreted the positive components of these double tracks as protons. The photographs were published on 18th December 1931.[2] This interpretation did not work with the second of the two photographs. The positive particle had little curvature; Millikan was forced to conclude that it was a proton of 450 mev, while the negative track was taken as an electron of 27 mev. At this energy the ionisation of a proton should be between 1·5 and 2 times that of an electron of 27 mev. But there was no detectable difference in ionisation between the positive and negative particles. This fact runs counter to certain well-established correlations.[3] Millikan thought there was something wrong with the theory of how the ionisation of protons varies with energy. He did

[1] Cf. *Electrons*, Chapter XIV, Figs. 54 and 55

[2] 'Cosmic Rays Disrupt Atomic Hearts' in *Science Service*

[3] Williams and Terroux, *Proc. Roy. Soc.*, 1929–30, **126**, 300

not then, nor did Anderson, ever suppose that the positive particle might be other than a proton. Millikan and Anderson agreed to come back to this 'proton' to discover the 'error'. But even now they knew that since an isolated single track of small ionising power curved rapidly to the right, and hence ought to be a positive particle going down, the track might also be considered as having been made by a negative going up. It was necessary to have two tracks branching downward from a common centre for any positive identification of a positive as opposed to a negative particle.[1] Only a small percentage of all the particles photographed by Anderson and Millikan were so 'associated'. Therefore, the 'proton' interpretation of the second photograph was difficult to check. Moreover, for reasons to emerge, the assumption that the fundamental unit of positive electricity *could only* be associated with the proton, made active challenging of the Millikan-Anderson interpretation seem *prima facie* implausible.[2]

[1] Cf. Skobeltzyn, *Zeitschrift für Physik*, 1929, **54**, 686; this is the first known to such branch tracks.

[2] Concerning this work of Millikan and Anderson, Professor Skobeltzyn has kindly sent me correspondence of the period from L. H. Gray, M. Curie, and F. Joliot-Curie. The relevant excerpts are set out here with the permission of the authors or their heirs or executors.

Cambridge, Nov. 27th 1931

Dear M. Skobeltzyn,

On Monday we had a visit from Millikan . . . he showed eleven photographs of 'good' tracks . . . which from the sense of their curvature must have been positive particles . . . In some cases a proton (?) and electron appeared on the same photograph, and with the help of a vivid imagination one *might* conclude that they had a common origin . . . in every case the proton tracks were more dense than those of the electrons. [This was not in fact true. But that Gray should have expressed himself thus is significant for our thesis.—N. R. H.] Their curvature was clearly visible in some cases, though not of course large . . . the general feeling was that the curvatures could hardly have been spurious . . . A large proportion of double tracks, as you had previously found, is in itself very significant . . .

Yours sincerely
L. Harold Gray

Paris, le 21 Décembre 1931

Cher Monsieur,

. . . Le point le plus important est que sur ces clichés [of Professor Millikan] on voit, non seulement des électrons, mais aussi des protons (déviation en sens inverse), et qu'on a vu, en particulier, des trajectoires d'électron et de proton paraissant partir d'un même point. Les potentiels indiqués par les courbures seraient de l'ordre de 100 millions de volts . . .

Veuillez agréer, Cher Monsieur, l'assurance de mes meilleurs sentiments.

M. Curie

Then came the photograph of 2nd August (cf. above). After encountering some further unambiguous cases Dr Anderson published his discovery.[1]

Nonetheless, the appearance of 'free' positrons continued to be regarded as a rare and peculiar event, even by Anderson. Indeed, it is reliably reported that when Bohr and Rutherford heard of Anderson's publication, they were full of scepticism. Prior theoretical commitments prepared them to feel *a priori* that a wrong interpretation must have been given to whatever it was that Anderson photographed. So most high energy positive tracks encountered during subsequent months were interpreted as protons. During all this rigorous questioning and reinterpretation, the best photograph remained the original one, that of 2nd August 1932.

In early 1933 Anderson published his most notable report, 'The Positive Electron'.[2] There, besides reproducing the original photograph, and setting out his hard reasoning in favour of a positron—reasoning quoted earlier—Anderson set out also a photograph appearing as follows:

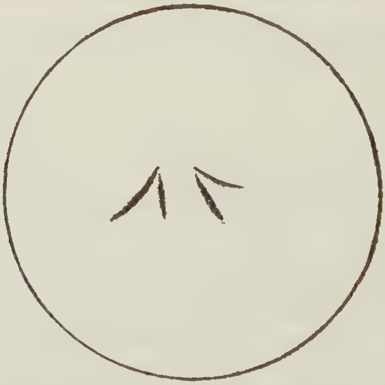

Fig. 4.

Mon cher Skobeltzyne, Paris, le 1^{er} Janvier 1932

. . . ses collaborateurs [i.e., Millikan's] ont photographiés des trajets de rayons cosmiques dans un champs magnétique élevé (13,000 G) et ils ont vu que certaines trajectoires sont dues à des électrons et d'autres à des protons. Il a même projeté un cliché d'où l' on voit issu du même point un électron et un proton d'énergie considérable 50×10^6 ev environs . . .

F. Joliot-Curie

And Professor Skobeltzyn himself remarks that as early as 1931 he 'prior to others, observed electron-positron pairs, not being able to identify them, however. (My note in "Nature" v. 133, p. 23, 1934.)' [Letter to the author, 22nd October, 1960.]

[1] In *Science*, September 1932, **76**, 238

[2] C. D. Anderson, *Physical Review*, 1933, **43**, 491; cf. also 1034

The outside (far left and far right) tracks are ' associated in time '. That is, they represent ionisations which certainly occurred at the same instant; they are all diffuse to the same degree.[1] The track at the extreme left is of a negative electron of 18 mev. That at the extreme right is of a positive electron of 20 mev. The diffuseness of both tracks results from the cloud chamber expansion having occurred $\frac{1}{2}$ second after the passage of the ionising particles. The ions have thus diffused away from the original track before being stopped by water vapour condensation. Hence, under a microscope, one can accurately count the ions per centimetre of path, as well as measure the curvature. These two considerations determined both the charges and masses of the two particles as exactly the same to within 10 per cent and 20 per cent respectively.[2] Within these limits positive and negative electrons now seemed ' identical twins ', the only difference being the sign of their charge. Indeed, in September of 1933, Anderson writes:

> The striking fact that in the cosmic rays positives and negatives occur in practically equal numbers and have apparently a similar energy distribution is in accord with the assumption that the nuclear effects involved give rise to the positives and negatives in pairs (in some cases several pairs as evidenced by the showers) . . . *Phys. Rev.* 1933, **44**, 416.

This, then, constitutes part of the chronology of the discovery of the Anderson positron. Fully to appreciate the concentration, and the challenge to theoretical orthodoxy which Anderson's interpretation of his data required—this will only begin fully to come out as one explores the contexts of two further discoveries. These were the discovery of the Dirac particle, and the Blackett-Occhialini particle.

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[1] These tracks are geometrically associated as well, and hence necessarily time-associated. Given the relative infrequency of cosmic-ray tracks in 1931 (1 in 30 exposures) the likelihood of independent events originating from a common centre is vanishingly small.

Compare Anderson, writing in 1933: ' The tracks . . . were coincident in time as shown by the fact that the diffusion of the ions broadened all the tracks to the same extent . . . Seven of the tracks were seen to originate at a common point in the upper portion of the chamber . . . The sense of curvature . . . is such as to indicate electrons . . . Protons . . . of the degree of curvature observed would have energies too low to be consistent with the observed minimum ranges and specific ionization . . . Obviously electrons cannot be the immediate ionizing agents . . . since more than 1000 electrons would have to be assumed to traverse the ion-chamber simultaneously . . . ' (*Phys. Rev.*, 1933, **43**, 369).

[2] Cf. ibid., 1933, **44**, 406

2 *The Dirac Particle*

The new quantum mechanics of Schrödinger and Heisenberg was not in agreement with experiment during late 1927 and early 1928. When applied to the atom with point-charge electrons, it failed to account for duplexity phenomena, and the empirically discovered spin-angular-momentum ($\frac{1}{2}$ quantum).[1]

In one of the greatest theoretical papers in the history of physics, Dirac begins by saying:

> One would like to find some incompleteness in the previous methods of applying quantum mechanics to the point-charge electron such that, when removed, the whole of the duplexity phenomena follow without arbitrary assumptions . . .

He goes on:

> . . . the incompleteness of the previous theories [lies] in their disagreement with relativity, or alternatively, with the general transformation theory of quantum mechanics.[2]

In a most effective contribution to the new quantum mechanics Gordon assumes that the quantum-theoretic operator of the wave equation should be got from the classical relativity Hamiltonian by putting into F, $W = ih\,(\delta/\delta t)$, $pr = -ih\,(\delta/\delta x_r)$, $[r = 1, 2, 3]$.

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[1] This paragraph is virtually a paraphrase of Dirac's prefatory remarks in the 1928 paper alluded to in the next footnote. When Professor Heisenberg read this paragraph, however, he reacted as follows: '. . . I cannot quite agree with the first four lines of Chapter II. . . . Here your report neglects completely the fact that at this time the theory of the electronic spin had been well-established. The multiplet structure of the atomic spectra did not at that time offer any essential difficulty for quantum mechanics. Both the multiplet structure and the anomalous Zeeman effect were well understood, and Pauli had introduced the Pauli spin matrices with the well known properties: $\mathscr{C}_x\mathscr{C}_y = -\mathscr{C}_y\mathscr{C}_x = {}_i\mathscr{C}_z$, etc. I cannot doubt that Dirac had been led to his discovery by Pauli's paper and especially by the relation $(p_x\mathscr{C}_x - p_y\mathscr{C}_y - p_z\mathscr{C}_z)^2 = p^2$. The essential progress in Dirac's paper was the connexion of Pauli's spin matrices with the Lorentz group. Dirac succeeded in representing the complete Lorentz group by introducing besides the Pauli spin matrices another similar matrix ζ which in some way represents the space reflections in the Lorentz group. It is the introduction of this ζ matrix which finally led to the dualism between matter and antimatter . . .' [Letter to the author, 19th September 1960].

[2] P. A. M. Dirac, 'The Quantum Theory of the Electron', *Proc. Roy. Soc.*, 1928, **117**, 610. Cf. also Pauli, *Zeitschrift für Physik*, 1927, **43**, 601; and Darwin, *Proc. Roy. Soc.*, 1927, **116**, 227.

This gives

$$F\psi = [(ih\,(\delta/c\delta t) + e/c\,(A_o))^2 + \Sigma_r\,(-\,ih(\delta/\delta x_r) + \\ + e/c\,(A_r))^2 + m^2c^2]\,\psi = 0 \qquad (1)$$

ψ being a function of X_1, X, X_3, t.[1] Gordon's equation was valuable and influential. But it involved two difficulties which Dirac remarks with clarity. The first concerns the physical interpretation of ψ. Gordon and Klein[2] made the aforementioned assumption, and it succeeded with the emission and absorption of radiation. But it was not general enough to answer the question 'What is the probability of a dynamical variable at t having a value lying between specified limits, when the system is represented by a given wave function ψ_n?'

> . . . the Gordon-Klein interpretation can answer such questions if they refer to the position of the electron . . . but not if they refer to its momentum, or angular momentum, or any other dynamical variable. We should expect the interpretation of the relativity theory to be just as general as that of a non-relativity theory. (Dirac, ibid. p. 612.)

The crux is this: in non-relativity quantum mechanics the wave equation is linear (of the first order in time) 'so that the wave function at any time determines the wave function at any later time. The wave equation of the relativity theory must also be linear in W if the general interpretation is to be possible' (ibid.). In short, Gordon's equation was not general enough: it could not give values to the dynamical variables when the system is represented by ψ_n.

The 1926 equation was of the second order in time (i.e. in $\delta/\delta t$). It is, of course, mathematically impossible to apply the general theory of transformations to differential equations higher than the first order in time.

So the first of the two difficulties Dirac perceives in the earlier Gordon equation consists in its not being of the first order in $\delta/\delta t$. He cannot be satisfied with such an equation.

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[1] Although written precisely as it appears on page 611 of Dirac's 1928 paper, scrutiny reveals that this equation (the Klein-Gordon) is improperly written. Dirac should have put a minus sign after the first square bracket, giving an equation of the form: $F = [-(W/c + e/c\,A)^2 + (p + e/c\,A)^2 + m^2c^2]\,\psi = O$. As they stand, equations (1) and (3) in Dirac's paper are formally inconsistent, a fact unnoticed during these thirty-three years. Professor Dirac has confirmed the present author's suggested emendation of (1), and of the equation in the middle of page 612. The latter appears unaltered in the quotation below, which begins 'The second difficulty . . .'. Again, a minus sign after the square bracket is necessary.

[2] Gordon and Klein, *Zeitschrift für Physik*, 41

> The second difficulty in Gordon's interpretation arises from the fact that if one takes the conjugate imaginary of [(1)] one gets
>
> $$[(-(W/c)+(e/c)A_0)^2+(-p+(e/c)A)^2+m^2c^2]\psi=0,$$
>
> which is the same as one would get if one put $-e$ for e. The wave equation (ibid. p. 611) thus refers equally well to an electron with charge e as to one with charge $-e$. If one considers for definiteness the limiting case of large quantum numbers, one would find that some of the solutions of the wave equation are wave packets moving in the way a particle of charge $-e$ would move on the classical theory, while others are wave packets moving in the way a particle of charge e would move classically. For this second class of solutions W has a negative value.[1] One gets over the difficulty on the classical theory by arbitrarily excluding those solutions that have a negative W. One cannot do this on the quantum theory, since in general a perturbation will cause transitions from states with W positive to states with W negative. Such a transition would appear experimentally as the electron suddenly changing its charge from $-e$ to e, a phenomenon which has not been observed. The relativity equation should thus be such that its solutions split up into two non-combining sets, referring respectively to the charge $-e$ and the charge e. (Ibid. p. 612.)

In short, the otherwise effective Gordon equation, besides being of the second order in time, also generated solutions involving particles with ' negative energy '. Such electrons had not been observed, or at least not identified, by 1928.

> In the present paper we shall be concerned only with the removal of the first of these two difficulties.

This last remark is historically interesting. Positron transitions are often referred to in the literature of theoretical physics as ' Dirac jumps '. But, considering the occasion for Dirac's problem, and his specific attack, it would be more appropriate to call such transitions ' Gordon jumps '. The original ' negative energy solutions ' were in print in 1926.[2] The problem was one of two which arose within the new quantum mechanics; it was the problem Dirac chose *not* to deal with in 1928. His problem was quite different. It was to ' obtain a wave equation of the form $(H-W)\psi=0$ which shall be invariant under a Lorentz transformation and shall be equivalent to [(1)] in the limit of large quantum numbers . . .' (ibid. p. 613).

The awe with which Dirac's 1928 paper was greeted by physicists was a function of the successful solution he found for this one problem.

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[1] Author's note: that is, these solutions have a ' negative energy '.

[2] Cf. Gordon, *Zeitschrift für Physik*, 1926, **40**, 117 ff.

In an elegantly powerful demonstration, he designs a new wave equation $(i\Sigma\gamma_{\mu}p'_{\mu} + mc)\,\psi = 0$, which is not only of the first order ; it can also be brought back into the form of the original Gordon equation, so that the important results of the latter stand out independent of the frame of reference used.'[1] Dirac immediately proves the new equation sufficient to account for all duplexity phenomena ; he works the spin-angular-momentum ($\frac{1}{2}$ quantum) into the logical structure of his new equation, instead of being, what it had been, a merely empirical adjustment, theoretically *ad hoc* and unconnected with general electron theory.[2] Beyond all this, Dirac's equation gave all the correct results for the Hydrogen atom; the Zeeman effect became calculable and explicable; the fine-structure formula comes out as a consequence; and the entire Dirac electron theory was applied [by Klein and Nischina] to the Compton scattering of electrons.

Theoretical physics had not witnessed such a powerful unification of concepts, data, theories, and intuitions since Isaac Newton proposed his Principle of Universal Gravitation. From a chaos of apparently unrelated facts and ideas, Newton in his way, and now Dirac in his, built a logically powerful and conceptually beautiful physical theory.

For this reason Dirac's paper is regarded by theoretical physicists as 'the greatest paper of our century'.[3] However, the second difficulty within the Gordon equation, and hence the Dirac theory, remained. Dirac felt this to be a serious blemish.

In the same way, and for the same reason, Dirac sought to deal with this perplexity, as Anderson and Millikan had originally tried to deal

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[1] The Dirac equation for an electron moving in an arbitrary electromagnetic field of potentials A_0, A_1 A_2, A_3, is thus of the form:

$$F\psi = [p_o + (e/c)\,A_o + \alpha_1(p_1 + (e/c)A_1 + \alpha_2(p_2 + (e/c)A_2) + + \alpha_3\,(p_3 + (e/c)A_3 + \alpha 4mc]\,\psi = 0 \qquad (2)$$

[2] This in no way minimises the great achievement of Goudsmit and Uhlenbeck (1925). Their brilliant hypothesis, however, was intended only to 'save the phenomenon', and not to strengthen electron theory. Cf. *Nature*, 1926, **117**, 264. Cf. Dirac: 'The αs [in (2)] . . . may be regarded as describing some internal motion of the electron, which for most purposes may be taken to be the spin of the electron postulated in previous theories . . .' (*Proc. Roy. Soc.*, **118**, 351) . . . '. . . We can interpret this result by saying that the electron has a spin angular momentum of $\frac{1}{2}hp$, which, added to the orbital angular momentum m, gives the total angular momentum M, which is a constant of the motion . . .' (*Proc. Roy. Soc.*, **117**, 620). And please compare Heisenberg's remarks on spin, introduced in the first note in this chapter. Cf. also 'Exclusion Principle and Spin' by B. L. Van der Waerden, in *Theoretical Physics in the Twentieth Century*, New York, 1960, especially pp. 232-233.

[3] This is Professor Hans Bethe's remark.

with theirs.[1] Dirac, following a suggestion of Weyl, tried to ' cook ' the negative-energy solutions so that they might be construed as protons.[2] Had he been successful, his paper might have seemed even greater, in 1928. For then, it would have been the complete theory of elementary particles ; protons and electrons were then thought to be ' all there were '.

But then Weyl[3] demonstrated this interpretation to be untenable. Whatever might be the further properties of the ' negative energy ' particles, they had to have the same mass as the negative electrons. This was built into the logic of the Dirac paper, and this could not be tinkered with without wrecking the symmetry of the entire argument. So any appeal to protons was now blocked.

At this time, Gamow, in his serio-comic manner, dubbed the new particles ' donkey electrons '. Taking them to be electronic in every other way, Gamow simply accepted that they had negative energy. Like donkeys, the harder you pushed them away, the more forcefully they came at you. Hence, a direct hit with a γ-ray photon, should send the donkey electron ' recoiling ' back into the source with high velocity. Thus Gamow's suggestion!

But this hypothesis, and the negative energy theory in general, was physically unintelligible. It hardly helped the experimenter who had just witnessed an electron falling back into the source to be told that it had negative energy.

Accordingly, Schrödinger sought to modify the Dirac equation.[4] He aimed to restructure the algebra so as to block such awkward consequences, but not in so transparently *ad hoc* a way as to constitute a mere rejection. Although at first promising, the results were shown, I think by Oppenheimer, not to be Lorentz-invariant. It is philosophically interesting to realise that this lack of relativistic invariance was, by itself, enough to mark down the Schrödinger modification as a failure. It was more important to theoreticians to have an equation which, although it harboured unintelligible solutions, was none the less Lorentz-invariant, than to adopt an alternative equation which, although easily interpretable physically, lacked in variance. The requirements of ' operationalism ' alone are sometimes not quite enough to force a change in a broad-based theory.

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[1] At first Dirac simply excluded these solutions, *ad hoc*: ' Since half the solutions must be rejected as referring to the charge $+ e$ on the electron, the correct number will be left to account for duplexity phenomena . . . ' op. cit. p. 618. And even in 1930 Oppenheimer writes: ' . . . since the Dirac jumps do not seem to be *directly* responsible for the difficulties with which we are, in this work, most concerned, we shall for the present neglect them ' (*Phys. Rev*,. 1930, **35**, 461).

[2] In this connection Dirac has written to me: ' The world as we see it is not symmetrical between positive and negative electricity, so a theory that makes it symmetrical would seem to be at fault. That is why I tried to cook the negative energy solutions. The lack of symmetry is still not adequately explained.' (Letter to the author, 1st November 1960.)

[3] Weyl, *Gruppentheorie und Quantenmechanik*, 2nd edn., p. 234

[4] Schrödinger, *Berl. Ber.* (1931), S. 63

Now the negative-energy solutions began to worry Dirac in earnest. They could not be rejected, nor written out of the theory, à la Schrödinger; the very design of an otherwise successful equation would collapse by either such attempt. Nor could they merely be christened ' donkey electrons ', possessed of ' negative energy '; this solves nothing since the idea is unsound. Nor could these solutions be ' cooked ' into protons, as Dirac once hoped; Weyl showed that. So these perplexing solutions could no longer be lived with comfortably.

In 1930 Oppenheimer echoes the march of Dirac's thought.

> . . . Dirac has suggested [*Proc. Roy. Soc.*, 1930, **126**] that the reasons why the transitions of an electron to states of negative energy, which are predicted by his theory of the electron, do not in fact occur is that nearly all the states of negative energy are already occupied. Dirac has further shown that the unoccupied states of negative energy have many of the properties of protons . . . they may be represented by wave functions which would be taken to correspond to a particle of positive charge and positive mass . . . the mass associated with these gaps is not necessarily the same as that of the electron, and he has suggested the assumption that the gaps are protons. . . . According to Dirac the scattering takes place by a double electron jump, in which a negative electron [i.e. a negative-energy electron] jumps up into some state of positive energy and the original positive [i.e. ordinary] electron falls down into the gap left. There are several grave difficulties which arise when one tries to maintain the suggestion that the protons are the gaps of negative energy, and that there are no distinctive particles of positive charge. . . .[1]

Oppenheimer here refers to Dirac's ' proton-cooking ' phase; he also is on the verge of indicating, what Weyl was also concerned to do, that this move cannot work.[2]

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[1] Oppenheimer, ' On the Theory of Electrons and Protons ' *Phys. Rev.*, 1930, **35**, 562

[2] The main reasons why this won't work are that (1) If it did one would have to give an infinite density to positive electricity, (2) the scattering of soft light by a

> If we return to the assumption of two independent elementary particles, of opposite charge and dissimilar mass, we can resolve all the difficulties raised in this note, and retain the hypothesis that the reason why no transitions to states of negative energy occur, either for electrons or for protons, is that all such states are filled. In this way, we may accept Dirac's reconciliation of the absence of these transitions with the validity of the scattering formulae.[1]

Slightly earlier, Oppenheimer had expressed his attitude toward the ' Dirac jumps '.

> On the present theory there is no normal state for matter, because states of infinite negative energy are possible; one may in fact show that, on the present theory Dirac jumps to such states from states of positive energy—jumps in which the energy and momentum lost by the matter are taken up by the field—are not only possible, but infinitely probable. But that the theory should have predicted this is a token of an error in the theory. . . .[2]

It is to this ' error in the theory ' that Oppenheimer, in the previous quotation, addressed himself.

The Dirac conjecture responsible for Oppenheimer's reactions can now be set out :

> The most stable states for an electron (i.e. the states of lowest energy) are those with negative energy and very high velocity. All the electrons in the world will tend to fall into these states with emission of radiation. The Pauli exclusion principle, however, will come into play and prevent more than one electron going into any one state. Let us assume that there are so many electrons in the world that all the

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proton is unaccountable-for on the Dirac theory; ' . . . [the Dirac] theory gives equal scattering coefficients for electron and proton . . . such interaction would affect electron scattering and proton scattering in exactly the same way; whereas the Thompson formula requires the latter to be smaller by a factor proportional to the square of the ratio of the masses ', and (3) there is a numerical discrepancy in the original Dirac supposition; a mean life for ordinary matter of the order of 10^{-10} seconds. ' We should hardly expect any state of negative energy to remain empty ' (ibid. p. 563).

Cf. also Oppenheimer, *Phys. Rev.*, 1930, **35**, 939: ' . . . we compute the rate at which electrons and protons should, on Dirac's theory of electrons and protons, annihilate each other '; . . . ' The mean lifetime of an electron in a proton density N_p proteins per unit volume is thus $T = (m^2/c^3/16\,\pi\,5\,e^4\,n_p) \sim 5 \times 10^{10}/n_p$ sec . . . an absurdly short mean lifetime for matter ' (p. 943). Cf. also Dirac, *Proc. Roy. Soc.* 1931, **133**, 61.

[1] loc. cit.

[2] Oppenheimer, ' Interaction of Field and Matter ', *Phys. Rev.*, 1930, **35**, 461

> most stable states are occupied, or more accurately, that all the states of negative energy are occupied except perhaps a few of small velocity. Any electrons with positive energy will now have very little chance of jumping into negative-energy states, and will therefore behave like electrons are observed to behave in the laboratory. We shall have an infinite number of electrons in negative-energy states and indeed an infinite number per unit volume all over the world, but if their distribution is exactly uniform we should expect them to be completely unobservable. Only these small departures from exact uniformity, brought about by some of the negative-energy states being unoccupied, can we hope to observe.[1]

Note Dirac's final remark. Here, in January of 1930, he is entertaining what would be necessary in order to make observations of 'negative energy states' which he now felt obliged to accept, since his electron theory refused to settle for less. It is the outside possibility of such observations that Dirac is now beginning to take seriously.

In 1930 and 1931 Oppenheimer, and Heisenberg (while writing on Halogen-theory), began to speak of these small departures from exact uniformity as 'vacancies in the negative-charge continuum'. These thinkers, along with Dirac, began to think of the $+\ e$ solutions of 1928 not so much as negative energy solutions, but simply as positive charge solutions. By early 1933 Blackett was confidently referring to the 'vacancies' as 'holes'.[2] These 'holes', gaps in an infinite sea of negative electricity, were taken now to be ordinary particles with positive kinetic energy and positive charge. It was already understood that they had the mass of an electron. The conclusion, therefore, was inescapable; the 'negative-energy solutions' heralded nothing other than electrons with a positive electric charge.

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[1] Dirac, *Proc. Roy. Soc.*, January 1930

[2] The idea behind the 'hole' theory is first explicitly put forward by Dirac, *Proc. Roy. Soc.*, 1930, **126**, 360; and *Proc. Camb. Phil. Soc.*, 1930, **26**, 361. In these papers the 'proton-cooking' occurred. For those to whom the conception of a positron as a 'hole in the negative-charge continuum' is difficult, the following may help. Imagine a sardine swimming through an infinite sea. This is in itself no feat of thought. But now 'reverse the Gestalt'. Instead of an infinite sea of water, imagine an infinite sea *of sardines*! They are so densely packed that they form a virtual continuum. These sardines now correspond to the most stable states for electrons ; each sardine is thus 'a negative energy particle of very high velocity'. Imagine now one sardine knocked out of its place in the sea. One would perceive a sardine-shaped vacancy in the sardine-sea, a hole which would stand out from the dense background as being something exceptional, just as a small hole in a large blackboard is immediately apparent. Suppose a sardine immediately adjacent to the hole moves into it, and that the next sardine moves into the new hole, and so also with the next, and the next. One *could* describe this as I have done. Or one *could* think of a sardine-shaped hole itself moving through the continuum. The motions of this hole would then be observed as clearly as would have been the motions of the original sardine in its infinite sea of water. And should any other sardine come skimming along the surface of the sea, it will, when it approaches

This shift of ideas percolated only slowly into the strange seas of physical thought in 1931. Thus Halpern and Thirring write :

> Dirac's system of equations refers to particles of charge $+ e$ as well as to those of charge $- e$; but this means that it has too many solutions for the charge $+ e$. As neither $+ e$ nor $- e$ has an advantage over the other, exactly half of the solutions refer to a positively charged particle and the other half to a negatively charged particle. Since in general two different solutions may be combined together, that is, a particle can pass from a state characterized by a proper function belonging to $+ e$ to the state characterized by a proper function belonging to $- e$, this signifies that according to Dirac's theory the electrons can change their sign. This is an effect for which no experimental evidence has ever been observed. It can also be shown that in the same process, namely, in the transition of the charge $+ e$ to $- e$, the sign of the total energy E must change. . . . Dirac's theory leads to the quite unintelligible result that particles with *negative mass* must exist. . . .
>
> In practice, therefore, until these difficulties are removed we shall merely retain one-half of the solutions which correspond to positive mass and subject them to comparison with experiment. That this comparison comes out satisfactorily in every respect was mentioned in the introduction. . . .[1]

Bolder is Oppenheimer's remark of 1930.

> . . . according to Dirac not all of the states of negative energy are full; there are a few gaps in the distribution for negative electrons merely at rest; and thus transitions to states of negative energy should not be quite excluded.[2]

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the hole, immediately fall into it. This will remove both that sardine, and the hole it fell into, from further observation. Only the featureless continuum will again be present; the sardine and the hole will have annihilated each other by the former's collapsing into the latter.

[1] Halpern and Thirring, *The Elements of the New Quantum Mechanics*, London, 1930–31, p. 101. And compare Condon and Mack, *Phys. Rev.*, 1930, **35**, 382: '. . . According to [Dirac's theory] electrons can exist in states of negative energy (less than $- mc^2$), as well as in usual states of positive energy (of the order of $+ mc^2$). We have no experimental evidence of electrons of this sort, and so the prediction of such negative energy electrons was regarded as a blemish in the theory. . . .'

[2] Oppenheimer, *Phys. Rev.*, 1930, **35**, 939

Even as late as 1933, W. Pauli argued that the Dirac equation was a wonderful contribution to microphysics; however, it entailed the existence of positive electrons; hence, the equation must be erroneous.[1]

Thus, the ‘negative-energy’ solutions, plus the rejection of their interpretation as protons, plus the 1930–32 ‘hole-theory’, of Dirac, Heisenberg and Oppenheimer—all this constituted the gradual development of a prediction of the existence of positive electrons before they were observed. The context remained full of uncertainties and incalculables. Nonetheless, the strength of Dirac's electron theory, plus the logic of its consequences, forced theoreticians to opt for the as-yet-unobserved entity.

This development is what inclines theoreticians to characterise the discovery as a theoretical breakthrough. The reader is left to decide whether the guerdon should be given to the theoretical prediction made in the total absence of a confirming observation, or to the frank interpretation of an awkward observation in the total absence of a theory to explain it. Doubtless either undertaking required uncommon courage, skill, conviction and care. Doubtless also, both Carl Anderson and Paul Dirac possessed these characteristics to a remarkable degree. This is shown by the fact that, in entire independence of each other, and against the best-established reasons to the contrary, they took their stand on the existence of a positively charged electron. Let us conclude this section with a quotation from Dirac himself, as he writes (in 1931) of the steps leading up to his great prediction:

> The mathematical formalism at that time involved a serious difficulty through its prediction of negative kinetic energy values for an electron. It was proposed to get over this difficulty, making use of Pauli's Exclusion Principle which does not allow more than one electron in any state, by saying that in the physical world almost all the negative-energy states are already occupied, so that our ordinary electrons of positive energy cannot fall into them. The question then arises as to the physical interpretation of the negative-energy states, which on this view really exist. We should expect the uniformly filled distribution of negative-energy states to be completely unobservable to us, but an unoccupied

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[1] Pauli, *Handbuch der Physik* (1933), Chap. 2, § 5: Ubergänge zu Zuständen negativer Energie, Begrenzung der Diracschen Theorie' (pp. 242 ff). Professor Skobeltzyn remarks: ‘This view [of Pauli's] prevailed in the opinion of our theoreticians up to the discovery of C. Anderson and Blackett and Occhialini.’ [Letter of 22nd October 1960.] Note how this contrasts with the earlier-cited opinions of Bethe and Konopinski, to the effect that this was primarily a theoretical discovery, not an experimental one.

> one of these states, being something exceptional, should make its presence felt as a kind of hole. It was shown that one of these holes would appear to us as a particle with a positive energy and a positive charge and it was suggested that this particle should be identified with a proton. Subsequent investigations, however, have shown that this particle necessarily has the same mass as an electron [1] and also that, if it collides with an electron, the two will have a chance of annihilating one another much too great to be consistent with the known stability of matter.[2]
>
> It thus appears that we must abandon the identification of the holes with protons and must find some other interpretation for them. Following Oppenheimer,[3] we can assume that in the world as we know it, all, and not merely nearly all, of the negative-energy states for electrons are occupied. A hole, if there were one, would be a new kind of particle, unknown to experimental physics, having the same mass and opposite charge to an electron. We may call such a particle an anti-electron. We should not expect to find any of them in nature, on account of their rapid rate of recombination with electrons, but if they could be produced experimentally in high vacuum they would be quite stable and amenable to observation. An encounter between two hard γ-rays (of energy at least half a million volts) could lead to the creation simultaneously of an electron and anti-electron, the probability of occurrence of this process being of the same order of magnitude as that of the collision of the two γ-rays on the assumption that they are spheres of the same size as classical electrons. This probability is negligible, however, with the intensities of γ-rays at present available.
>
> The protons on the above view are quite unconnected with electrons. Presumably the protons will have their own negative-energy states, all of which normally are occupied, an unoccupied one appearing as an anti-proton.

In this passage Dirac sharply distinguishes the proton from the positron. He states that the latter, as well as the former, is in principle amenable to observation, and predicts circumstances under which positrons might be detected. He entertains seriously the ‘ materialisation ’ hypothesis later discussed by Joliot-Curie: this is nothing less than the creation of matter out of energy, one of the boldest conjectures of all time. And he considers the possible existence of an anti-proton. All this in less than forty lines of prose!

(*to be concluded*)

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[1] H. Weyl, *Gruppentheorie und Quantenmechanik*, 2nd edn., p. 234 (1931)

[2] I. Tamm, *Z. Physik*, 1930, **62**, 545; J. R. Oppenheimer, *Phys. Rev.*, 1930, **35**, 939; P. Dirac, *Proc. Camb. Phil., Soc.*, 1930, **26**, 361

[3] J. R. Oppenheimer, *Phys. Rev.*, 1930, **35**, 562

THE PREJUDICE OF MICRO-REDUCTION *

G. Schlesinger

I

There is a powerful tendency to take it for granted that physical order and logical order correspond to one another. We often yield to the prejudice that the physical relationship of things determines the logical relationship of propositions referring to them. It is usually assumed for instance that propositions describing the properties of a physically 'complex' whole are logically more 'complex' than those referring to the properties of its constituent parts. Statements about the behaviour of the 'elements' or 'simple' parts of a material system are regarded as if of necessity also logically more 'elementary' or 'simple'.

In fact, however, the logical relationship between propositions is indeterminate as such. For logical relationship can only be specified in the context of some deductive system. It depends on the premises and postulates of a particular logical system whether or not, relative to it, any two given propositions are interdependent and are consistent with one another. In case both propositions are derivable in this theoretical system it entirely depends on the logical structure of this system which proposition will feature amongst the early theorems, and hence be more 'elementary' or 'simple' and which one amongst the more advanced or 'complex' theorems.

To explain the nature of this deep-rooted conviction of the existence of parallel orders between the physical and the logical, and to show that it lacks objective foundation I shall first examine a recent paper by P. Oppenheim and H. Putnam—'The Unity of Science as a Working Hypothesis'.[1] I have chosen this paper with which to begin my discussion because the view it represents is a widely shared scientific ideology. It also has the advantage that, unlike most people, the authors do not merely take the correspondence for granted but state it in clear terms and boldly support it with arguments. This renders their thesis more vulnerable to criticism, and it becomes a

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* Received 27. ix. 60

[1] *Minnesota Studies in the Philosophy of Science*, vol. 2, pp. 3-35

comparatively easy task to expose the sentiments voiced by them, as objectively unfounded.

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Oppenheim's and Putnam's major thesis is a familiar one: the sciences can be arranged in a hierarchy ordered according to the complexity of the entities with which they deal. The authors divide these entities into six groups corresponding to six levels of complexity. Entities belonging to a given level are comprised of entities belonging to the level immediately below them: Level 6—Social Groups; Level 5—(Multicellular) Living Things; Level 4—Cells; Level 3—Molecules; Level 2—Atoms; Level 1—Elementary Particles. At present, entities of each level are described by predicates of which some belong exclusively to that level. The entities are governed by laws whose study constitutes, in part, the various special disciplines. It is hoped, however, that the special concepts and laws associated with the different levels will eventually be disposed of, and the various sciences dealing with them be replaced by a single unifying science.

The eventual unification is envisaged by the authors as taking place by a method they have named ' micro-reduction '. It consists in the breaking up of a system into its elements and explaining the properties of the whole in terms of the properties of its parts. The properties of entities of level $n + 1$ will therefore be explained in terms of the properties of entities of level n, and these in turn will be explained in terms of the properties of objects belonging to level $n - 1$. Thus the only science which will remain, after the reduction of all the others to its level, will be the science of sub-atomic physics.

The authors' argument in favour of the adoption of their programme may be divided into three parts, (*a*) that the unity of science is a desirable working hypothesis, (*b*) that the possible objections to its attainability can be met, and (*c*) that there is positive evidence to support the claim that it will be attained.

The essence of (*a*) is that the unification of science will bring about an enormous simplification, and no one will want to quarrel with this. That the correlation of the laws governing various phenomena renders science more manageable and simple is a truism. This creates no special grounds, however, on which to prefer micro-reduction to any other method of unification. The essential prerequisites for the correlation of the laws governing the behaviour of different kinds of entities are: (1) that the relevant properties of the parts be governed by regularities expressible by a group of propositions which I shall denote by P_1, (2) that the relevant properties of the whole be governed by regularities expressible by a group of propositions which I denote by P_2, and (3) that we be able to construct a theory consisting of a reasonable number of premises and postulates (a small part of P_1 and P_2 may feature amongst them) in which P_1 and P_2 can logically be derived. Whether this theory is of the type T_1, namely that P_1 features amongst the earlier theorems of the deductive system, or of the type T_2, in which case P_2 constitutes the elementary part of the theory, has no bearing on the amount of economy achieved. The simplification introduced by T_2 (by macro-reduction) may be just as great as by T_1 (by micro-reduction).

Of the arguments of interest that come under (*b*), one deals with the objection that their programme neglects the fact of emergence. According to the thesis of emergence when objects of a given level combine to form wholes belonging to a higher level, the new properties that emerge are not reducible to the properties the elements possess when separated. Oppenheim and Putnam argue, against this, that though in practice there are many properties that seem emergent, no one has so far produced positive arguments to show that reduction is impossible in principle. Our inability at the moment to ' micro-reduce ' all phenomena should not be construed as positive evidence against the working hypothesis that this will eventually be possible.

Their stand against the dogma of emergence seems very reasonable. It should be pointed out, however, that Oppenheim and Putnam, like almost everyone else who has discussed the concept of emergence, take it for granted that properties of wholes are either emergent or, if not, they are what we now call ' micro-reducible '. But, in fact, the proper alternatives are: the thesis of emergent phenomena (or the incorrelativity of phenomena) on the one hand, and the thesis of the connectibility of phenomena on the other. For correlation may be achieved in a variety of ways. In other words, if no theory can be constructed in which both P_1 and P_2 are derivable, then the properties of the whole are to be regarded as emergent. On the other hand, if such a theory can be constructed, then no matter whether it is of type T_1 or T_2, the properties of the whole are not emergent but connectible to those of its elements.

Of the arguments that come under (*c*)—the positive arguments in favour of the authors' plan—the one based on the theory of universal evolution is of major significance and interest. Oppenheim and Putnam quote the views of a large number of authorities to show that their table of reductive levels represents a hierarchy of evolutionary levels. Whatever belongs to a higher level in their scheme came into existence at a later stage in history. They devote a section of their paper to showing that human beings lived as isolated individuals before societies came into existence; that, at an early time, only inorganic matter existed; and that there was even a period, lasting from five to thirty minutes after our universe began, in which there was nothing but sub-atomic particles. This, they contend, provides support for their thesis, for:

> Let us, as is customary in science, assume causal determination as a guiding principle; i.e. let us assume that things that appear later in time can be accounted for in terms of things and processes at earlier times. Then, if we find that there was a time when a certain whole did not exist, and that things on a lower level came together to form that whole, it is very natural to suppose that the characteristics of the whole can be causally explained by reference to these earlier events and parts; and that the theory of these characteristics can be micro-reduced by a theory involving only characteristics of the parts.[1]

This is a striking example of the tendency to identify causal order with logical order. Let me indicate, by means of a simple concrete example, where the error lies.

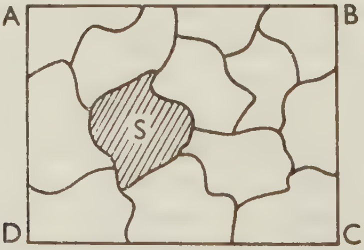

Fig. 1.

The uniform reflecting surface ABCD consists of curvilinear surfaces of which S is of typical shape. We may logically derive the value of the total reflection for ABCD from our knowledge of the separate values for all the S-surfaces, by assuming or postulating that the property of total radiation is additive.

There is, however, an alternative, indirect method whereby we may arrive at the value of the total reflection of ABCD but not in terms of any of the properties of its constituent elements, the S-surfaces.

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[1] Op. cit., p. 15.

If we know the reflective power of the kind of surface possessed by ABCD, we may use the formula: total reflection = reflective power × area. To calculate the area we employ the simple formula provided in Euclidean geometry.

If the S-surfaces are of such shape that the value of their area is derivable in Euclidean geometry, then the properties of total radiation of the whole and its parts may also be unified by the use of our last method. This unification is obtained within the framework of a typical T_2 theory. The theorem giving the area of an S-figure would be an advanced one presupposing many earlier theorems, including all those required for the demonstration of the formula: area of rectangle = length × breadth. In the context of Euclidean geometry, that aspect of the complex whole which is known as area is more elementary than that of its physical elements, the S-figures. The propositions about the respective values of the total reflection are not bridged by micro-reduction.

Now, no argument from evolution could show that in the T_2 theory the principle of causality had been violated. Imagine that at an early stage of evolution the whole universe consisted of S-surfaces which at a later stage combined to form the rectangular surfaces that are now in existence. It would clearly be pointless to argue that since historically the curvilinear figures preceded the rectangular ones, and since the latter were caused to come into existence by the conglomeration of the former, no T_2 theory can truly account for their present properties. The logical order of statements about the properties of different entities is independent of their physical order of succession. The former is determined solely by the structure of the unifying theory.

The argument from universal evolution has, however, a curious suggestive power. I have heard it defended in the following manner. Suppose for example that the properties of inanimate matter were explained by a T_2 theory, that is, they were macro-reduced and explained in terms of the properties of organic cells. This would imply that the behaviour of living cells determined the behaviour of molecules. Assuming that a cause cannot act except when it is and where it is, you would be forced to conclude that before there was life on earth—so that cells could not determine the behaviour of molecules—the behaviour of inorganic matter was indeterminate!

But this is merely a restatement of the same confusion between causal and logical determination. There is no reason why propositions about the behaviour of non-existent entities (and if you like of entities that will never exist) should not, in the context of a certain theory, logically determine or imply propositions about the behaviour of existing entities. Our firm belief in determinism would in no way diminish if we claimed that the behaviour of molecules was always governed by strict laws; however, these laws and the laws governing the behaviour of cells cannot be unified unless by a theory in which the latter constitute the more elementary theorems.

3

A further argument of Putnam and Oppenheim is as follows. Suppose that B_1 is that branch of science which deals with entities E_1 and B_2 that branch which deals with entities E_2, then:

> . . . it seems very doubtful to say the least, that a branch B_2 could be reduced to branch B_1, if the things in the universe of discourse of B_2 are not themselves in the universe of discourse of B_1, and also do not possess a decomposition into parts in the universe of discourse of B_1 (they don't speak about the same thing).[1]

Yet, consider the way the Boyle-Charles Law was ' micro-reduced ' to the laws of mechanics within the kinetic theory of gases. How could this be done, when the Boyle-Charles Law ' speaks about ' concepts like ' temperature ' that are not to be found in Newtonian Mechanics? As Ernest Nagel so clearly explained,[2] such reductions are accomplished by the introduction of a postulate of correlation, in this case that the thermodynamical ' temperature ' is proportional to the mechanical ' mean kinetic energy '. It is a mistake to think that, just as physical analysis will reveal a gas to consist of molecules, logical analysis will reveal that ' temperature ' means ' mean kinetic energy '. The laws governing macroscopic entities are not by themselves entailed by the laws governing microscopic entities even though the former entities physically result from the latter. This, of course, is true even in cases where, unlike the case of mechanics and thermodynamics, P_1 and P_2 employ the same set of concepts. Even the innocuous statement that the weight of a gas is the sum total of the weight of its constituent parts requires the postulate that weights are additive.

Certain features of common language may also be responsible for our thinking that, just as, say, societies possess a decomposition into individuals so do the concepts of sociology possess a natural decomposition into the concepts of individual psychology. I have already alluded to the suggestive nature of such double-meaning terms as 'elementary', 'simple' and 'complex'. To take a more conspicuous example, consider the term 'analysis'. According to the Oxford English Dictionary, 'analysis' is the act of proving a proposition by 'resolving' it into simpler propositions already proved or admitted. In the physical sense, the analysis of an aggregate is its resolution into its component particles. Both processes are denoted by the same word. Yet we must appreciate that this does not constitute a guarantee that they are parallel processes.

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[1] Op. cit., p. 8

[2] ' The Meaning of Reduction in the Natural Sciences ', *Science and Civilization*, edited by R. C. Stauffer, Wisconsin, 1949, pp. 99-135

'Speaking about the same thing' then is not ensured by the presence of particular physical set-up to which propositions refer. It is a logical feature varying with the theories (and their sets of postulates) in the context of which these propositions are viewed. There is no warrant therefore to conclude

> Thus one cannot plausibly suppose—for the present at least—that the behaviour of inorganic matter is explainable by reference to psychological laws; for inorganic material does not consist of living parts.[1]

for inorganic matter may not physically consist of living parts yet the laws of physics may be analysed into the laws of biology within the context of a theory in which the latter form the elementary parts.

4

It would seem perhaps that one might yet take a more modest stand in defending the thesis of micro-reduction. One might be prepared to admit that there are no *a priori* reasons to say that the properties of the whole and its parts, if at all connectible, are more likely to be connectible by a T_1-theory than by a T_2-theory and still wish to argue that it is more desirable that they be so connected. Thus even though there is no guarantee that in any particular case a T_1-theory can be constructed we should never abandon the search for such theory because of its superiority over a T_2-theory.

In support of this last claim one would cite what are commonly agreed the essential features of a good theory. A good deductive system, it is said, should have a minimum number of independent axioms or premises which should entail a maximum number of theorems. In a T_1-theory statements about the behaviour of the elements form the premises, which are but a few, and the more numerous statements describing the diverse behaviour of the aggregate are the entailed theorems. A T_1-theory therefore better confirms to the requirements of a good theory.

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[1] *Minnesota Studies in the Philosophy of Science*, vol. 2, p. 8

But the unwarranted assumption that has been introduced into this argument is the quite common notion that physical elements can never have more properties than the complex system that their assembly gives rise to. For an aggregate, it is argued, must at least possess all the properties of its parts and in normal cases some additional ones as well. A macroscopic body, for example, has mass, momentum, kinetic energy, but in addition, it has properties like temperature and viscosity which its microscopic elements do not possess.

This idea, which is perhaps most dramatically illustrated by the perennial search for the ultimate simple elements in nature, has, however, no logical basis. One can even demonstrate empirically that it is possible for elements to possess properties not shared by their aggregates. For example, portions of a system each by itself may sometimes have the property of being electrically charged hence possessing a disposition to exert a force upon uncharged bodies. When, however, they are put together, to form a system, their assemblage may become electrically neutral and devoid of such disposition.

Another example: curvilinear slabs have many dispositions rectangular slabs lack. One such disposition is to start rolling with different rates of acceleration when stood on different points on their circumference. But, in some cases, slabs which form the parts of a larger system may possess the property of having curvilinear edges, a property lacked by the whole formed by their conglomeration (as illustrated by Figure 1).

Apart from this we must realise also that the question of the relative merits of a T_1-theory and a T_2-theory is independent of the question whether the parts or the compound possess the larger number of significant properties. Not all the properties of the whole need to form the independent unproven and initial theorems of our deductive system, only some of them. The rest may be derivable from those and so may the properties of the part. Thus even if it were true that the properties of a complex system were always more numerous than those of its constituent elements, the argument introduced in this section to defend the prejudice of micro-reduction would be ineffective.

5

The conclusion reached is that there are no objective grounds on which to maintain that a T_1-theory will naturally be more readily available, or is more strictly in conformity with a deterministic view, or is for reasons of economy or elegance in any way more desirable, than a T_2-theory. There is just no reason why we should seek micro-reduction in preference to macro-reduction. Nevertheless it is a highly plausible claim that if the unification of science is eventually to be achieved, it will be achieved by micro-reduction. For the universal and deep-seated conviction in the thesis of micro-reduction will probably ensure that this will happen.

This conviction is reflected in common language, but it has also explicitly been voiced by philosophers of all ages. From Aristotle who said:

> . . . the compound should always be resolved into the single elements or the least part of the whole.[1]

till Wittgenstein who said:

> Every statement about complexes can be analysed into a statement about their constituent parts, and into those propositions which completely describe the complexes.[2]

philosophers assumed without much ado that the properties of the whole could and should always be explained in terms of those of their constituent elements, or what it all boils down to, that there is a natural intrinsic correspondence between physical order and logical order.

This conviction has had a tremendous influence on the course of science. Although macro-reduction has from time to time been attempted with some notable results[3] the vast majority of those theories in science which connect the behaviour of wholes with those of their parts are micro-reductive. The long history of the atom illustrates in a special way the great power of this conviction. In this case, the existence of a micro-reductive theory based on the few and simple properties of the parts in terms of which the manifold properties of the whole will be explained, was assumed, long before the existence of those parts could with any degree of credibility be maintained.

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[1] *The Basic Works of Aristotle*, New York, 1941, p. 1127

[2] L. Wittgenstein, *Tractatus Logico-Philosophicus*, London, 1922, 2.0201

[3] E.g. Stevin's derivation of the law of the composition and resolution of forces acting on a single particle from the behaviour of an endless chain, i.e. an aggregate of such particles. For the discussion of this and some other examples, see my ' Two Approaches to Mathematical and Physical Systems ', *Philosophy of Science*, **26**, No. 3.

The firm universal belief in the efficacy of micro-reduction, then, if nothing else, may bring about the realisation of the prognostications of Oppenheim and Putnam. For, as in the past, so in the future, we shall persist in exploring all the avenues that lead to the micro-reduction of phenomena.

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ARGUMENTS ABOUT MEANINGLESSNESS *

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Some philosophers have tried to set up a principle that would enable them to dismiss certain kinds of utterance as literally meaningless. In this paper we will show that some of the arguments used in this connection were unsound. From a logical point of view, the weaknesses in these arguments are of some interest. Philosophically the arguments may be out of date, but even if this is so their defects should still be noted, because philosophers have been known to retrace their steps.

'A sentence has literal meaning if and only if the proposition it expresses is either analytic or empirically verifiable.' Ayer says that it might be argued that the Principle of Verification, formulated in this way, is otiose, because a sentence that expresses any sort of proposition is meaningful. In order to deal with this difficulty, he decides to use the word 'statement' in a new way.[1] His proposals are as follows: any form of words that is grammatically significant is a sentence; every indicative sentence expresses a statement, and some but not all statements are propositions. Indicative sentences that express statements that are propositions are literally meaningful, and those that express statements that are not propositions are literally meaningless. Ayer retains the rule that all propositions and only propositions are either true or false, and for brevity he describes statements that are not propositions as being literally meaningless. Thus he is committed to saying that every statement has one of the three values: true, false, and literally meaningless.

Carl Hempel, in discussing the Principle of Verification, does not speak of statements or propositions but only of sentences, and for him it is indicative sentences that are true, false, or cognitively meaningless.[2]

In this paper we will usually abbreviate 'literally meaningless' and 'cognitively meaningless' to 'meaningless', and since the alleged difference between indicative sentences and statements is irrelevant, we

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* Received 4.x.60.

[1] A. J. Ayer, *Language, Truth and Logic*, 2nd edition, London, 1946, p. 8

[2] Carl G. Hempel, 'Problems and Changes in the Empiricist Criterion of Meaning,' *Revue internationale de Philosophie*, **11**, 1950, 41–63

will refer to whatever are true, false, or meaningless, as statements. The capital letters ' O ', ' P ', ' Q ', ' S ', sometimes with suffixes, will be used as the names of statements, and P $\vee$ Q, for example, is the name of the statement which is the disjunction of P and Q.

In talking about statements that are true, false, or meaningless, we make meta-language statements that are either true or false. The implication statements with which we shall be concerned, are such that their truth depends only upon the validity of truth functional schemata. For example, the statement ' Given *any* two statements P and Q, Q follows from P . P $\supset$ Q ' is true if and only if the schema $p \,.\, p \supset q \,.\supset q$ is valid. We assume that the validity of the appropriate truth functional schema is a condition not only necessary but also sufficient for one statement following from, being deducible from, being entailed by, or being a logical consequence of another. Some people hold that the validity of a material conditional is not a condition sufficient for an entailment, but here we use ' entails ' in the way in which it is used in the arguments that we examine, and in this context our assumption is justified.

2 First we will show that some arguments against certain formulations of the Principle of Verification are unsound, because in them a consequence of using ' statement ' in a new way is overlooked.

2.1 One formulation of the principle was that ' a statement is verifiable, and consequently meaningful, if some observation statement can be deduced from it in conjunction with certain other premises, without being deducible from those other premises alone ' [1] Ayer accepted the following argument to the effect that this allows meaning to any statement whatever. For any statement S, the observation statement O follows from S and S $\supset$ O without following from S $\supset$ O alone, and consequently by the above criterion, S is meaningful. But a necessary condition for O following from S and S $\supset$ O is that

$$p \,.\, p \supset q \,.\supset q \qquad \text{(i)}$$

comes out true for every interpretation of p and q. This schema is valid in the classical propositional calculus, but in this calculus letters may be interpreted only as being either true or false. If S is a statement in Ayer's sense of ' statement ', then it is not a possible interpretation of a letter in a schema in a two-valued logic, and to treat it as such is to beg the question by supposing that it is either true or false, and not meaningless.

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[1] A. J. Ayer, op. cit., p. 11

2.2 One of the criteria discussed by Hempel is that a statement is meaningful if and only if it is either analytic or self-contradictory, or follows logically from some finite and logically consistent set of observation statements (O).[1] We are asked to suppose that S and Q are statements that are not analytic or self-contradictory, and that S follows logically from O, and is consequently meaningful, but that Q is not meaningful. Hempel then argues that $S \vee Q$ is meaningful despite its meaningless component, because being a consequence of S, it is also a consequence of O, his objection being that an adequate criterion should rule out such statements as $S \vee Q$. Here it is being assumed that

$$p \supset q . \supset : q \supset r . \supset . p \supset r \qquad \text{(ii)}$$

$$p \supset . p \vee q \qquad \text{(iii)}$$

are valid in a logic in which the letters may be interpreted as being true, false or meaningless.

2.3 Hempel's argument is a part of what J. W. N. Watkins calls the tacking paradox.[2] Suppose we accept (1) that if a compound statement has a meaningless component, then it is meaningless, and (2) that if a statement is a logical consequence of a meaningful statement then it is meaningful. This, according to Watkins, leads to a contradiction, for $S \vee Q$ is meaningless because its component Q is meaningless, and it is meaningful because it is a logical consequence of the meaningful statement S. This is not a paradox. $S \vee Q$ would be a logical consequence of S and thus meaningful (by 2), only if $p \supset . p \vee q$ were true for all interpretations of p and q. But (by 1) $p \supset . p \vee q$ is not true if p or q is interpreted as meaningless. Thus it is possible to accept without contradiction or paradox that if a compound statement has a meaningless component, then it is meaningless (1), and that if a statement is not meaningful, then it is not a logical consequence of a meaningful statement (contrapositive of 2).

2.4 Ayer replaced the criterion mentioned in 2.1 by the following recursive one.[3] S is directly verifiable if and only if it is an observation statement, or such that, O_1 S entails O_2, where O_1 and O_2 are observation statements such that O_1 does not entail O_2. S is indirectly verifiable if and only if S P entails a directly verifiable statement not entailed by P alone, where P is a conjunction of statements each one of which is analytic, directly verifiable or independently indirectly verifiable. A literally meaningful statement is either analytic or self-contradictory, or directly or indirectly verifiable.

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[1] Carl G. Hempel, op. cit., p. 45

[2] J. W. N. Watkins, 'When are Statements Empirical?', *The British Journal for the Philosophy of Science*, **40**, 1960, 304

[3] A. J. Ayer, op. cit., p. 13

Church [1] claimed to show of any statement S that either it or its negation is directly or indirectly verifiable (under this definition), provided that there are three observation statements O_1, O_2, O_3, no one of which entails any other. $\overline{O}_1\ O_2 \vee O_3\ \overline{S}$ is directly verifiable because with O_1 it entails O_3. Since S and $\overline{O}_1\ O_2 \vee O_3\ \overline{S}$ together entail O_2, either S is indirectly verifiable, or $\overline{O}_1\ O_2 \vee O_3\ \overline{S}$ alone entails O_2, in which case $O_3\ \overline{S}$ entails O_2, and $\overline{S}$ is directly verifiable.

But for the first two entailments to hold generally it is necessary that

$$p \,.\, \bar{p}\, q \vee r \bar{s} \,.\, \supset r \qquad \text{(iv)}$$

is a valid schema. And the last step in the argument depends upon the validity of

$$\bar{p}\, q \vee r s \,.\, \supset q :\ \supset .\, r \bar{s} \supset q \qquad \text{(v)}$$

These schemata are valid in the classical propositional calculus, but in this calculus the letters may be interpreted only as being either true or false. Thus what Church has shown is that any true-or-false statement, that is to say, any meaningful statement, is such that either it or its negation is verifiable, which does not conflict with Ayer's recursive criterion.

3 Although the arguments discussed in 2 are unsound, it does not follow that the formulations of the Principle of Verification against which they are directed are satisfactory, particularly as nothing has been said to indicate that the formulations are not confused in just the way that the arguments are. In order to reassess both, it is necessary to take account of the new way in which ' statement ' is being used, by setting up a statement calculus in which the letters may be interpreted as true (1), false(0) or literally meaningless ($\frac{1}{2}$), and going on to see if the relevant schemata are valid in this calculus. This is not trivial, for if the notion of negation is extended so that the negation of a meaningless statement is meaningless, and if the rule is retained that a schema is valid if and only if it comes out true for all interpretations of its letters, then no matter how the relevant connectives are defined, some schemata that are valid in the classical propositional calculus will be invalid in the three-valued calculus (see below).

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[1] Alonzo Church, Review of Ayer's *Language, Truth and Logic* 2nd edn., in *The Journal of Symbolic Logic*, **1**, 1949, 53

In setting up this calculus we define connectives by means of matrices. Since we are merely extending existing notions of disjunction, conjunction and the material conditional in order to deal with statements that may be meaningless, we retain the values assigned to functions when their components are either true or false, and have only to assign values to functions when one or more of their components are meaningless. If F is a connective already defined for two values, then the values of a, c, g and i in

F	0	½	1
0	a	b	c
½	d	e	f
1	g	h	i

are retained. If ‘ F ’ is the material conditional sign, for example, then a, c, g and i are 1 1 0 1 respectively. We define such connectives for three values by specifying b, d, e, f and h. We denote the resulting matrix by the appropriate letter in the Polish notation followed by the values of b, d, e, f and h. Thus C 1 1 1 1 0 denotes the matrix

C	0	½	1
0	1	1	1
½	1	1	1
1	0	0	1

which defines the material conditional for three values.

What values are to be given to b, d, e, f and h is not obvious. Rosser and Turquette [1] have suggested that if truth values are regarded as indicating degrees of assertability, then a conjunction may be generalised as the function that is just as assertable as the least assertable of its components, and a disjunction as the function that is just as assertable as the most assertable of its components. But this method cannot be applied here, because whether a meaningless statement is to be regarded as being more or less assertable than a false one, is arbitrary.

A very simple way of producing a three-valued calculus would be to adopt the suggestion 2.3 (1) that a function is meaningless if it has one or more meaningless components (i.e. $b=d=e=f=h=\frac{1}{2}$ for all connectives), and to accept as valid any schema which does not come out false for any interpretation of its letters. All schemata valid in the classical propositional calculus would be valid in this system, but its adoption would lead to the following difficulty. Although the schema (iii), for example, is valid in this system, from the truth of S, it follows that S **v** Q is not false, but not that it is true. Also the substitution of statements for letters in a valid schema does not always give a true statement. Because of this second difficulty, we will retain the rule that a schema is valid if and only if it comes out true for all interpretations of its letters (R).

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[1] J. B. Rosser and A. R. Turquette, *Many valued Logics*, Amsterdam, 1952, p. 15

A method more suitable in this context is that used by Dienes.[1] He has worked out which of certain important characteristics of negation, disjunction, conjunction, and the material conditional and biconditional are retained, and which lost, when the matrix definitions of these connectives are extended in every possible way to cover a third value. Here his results are accepted and some of their consequences worked out and applied.

Unlike Dienes, who is not particularly concerned with interpretations of the third value ($\frac{1}{2}$), we intend to interpret it as ' literally meaningless ', and this enables us to choose one of the three possible extensions of negation. Although from some criteria of significance it follows that the negations of some meaningful statements are meaningless, this consequence has, as far as we know, always been regarded as a defect. This being so, we will take it that only the negation of a meaningless statement is meaningless, and adopt as negation the connective that Dienes calls N. He has shown that there are only two ways of extending conjunction and disjunction so that they continue to be commutative, associative, mutually distributive and de Morgan transforms of each other, these being $A\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}$, $K\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}$ and $A\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}\,1\,1$, $K\,0\,0\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}$. We now have to extend the material conditional in such a way that when combined with one of these pairs, it retains its more important characteristics. In adopting N, and in keeping the rule (R), we have precluded the possibility of retaining as valid all the schemata that are valid in the two-valued calculus, and consequently we have to decide what we require of an extension of the material conditional.[2] Dienes describes as ' the usual requirements ' the validity of six schemata including (i), (ii), and (iii). If we add (iv) and (v) to his list, we have:

(i) $p \,.\, p \supset q \,.\, \supset \, q$ *modus*
(i′) $p \, \supset \, : p \supset q \,.\, \supset q$ *ponens*
(ii) $p \supset q \,.\, \supset \, : q \supset r \,.\, \supset \,.\, p \supset r$ hypothetical
(ii′) $p \supset q \,.\, q \supset r \,.\, \supset \,.\, p \supset r$ syllogism

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[1] Paul Dienes, ' On Ternary Logic ', *The Journal of Symbolic Logic*, **2**, 1949, 85–94

[2] Paul Dienes, op. cit., 92, n. 7

(iii) $p \supset . p \vee q$

(iv) $p \;.\; \bar{p}q \vee r\bar{s} . \supset r$

(v) $\bar{p}q \vee r\bar{s} . \supset q : \supset . r\bar{s} \supset q$

(vi) $p \supset q . \supset . \bar{q} \supset \bar{p}$ contraposition

The following points also have a bearing upon definitions of the material conditional. Let X $\supset$ Y be a valid schema, and let P and Q be statements obtained from X and Y respectively by the same interpretation of letters. If in the definition of the material conditional $h = 1$, then from the premise that P is true we cannot conclude that Q is true, but only that it is not false. If $b = 1$ or $h = 1$, then from the premise that P is meaningful we cannot conclude that Q is meaningful. Thus in order to retain the rule that any statement implied by a true statement is true, we must, in defining the material conditional, give to h a value other than 1. The value of b is not as important, for although if $b = 1$ and $h \neq 1$, Q may be a meaningless statement when P is false, since the false antecedent cannot be asserted, the meaningless consequent cannot be detached.

The sets of definitions

$$(\alpha)\ \mathrm{N},\ \mathrm{A}\,\tfrac{1}{2}\,\tfrac{1}{2}\,\tfrac{1}{2}\,1\,1,\quad \mathrm{K}\,0\,0\,\tfrac{1}{2}\,\tfrac{1}{2}\,\tfrac{1}{2},\quad \mathrm{C}\,1\,1\,1\,1\,0$$

$$(\beta)\ \mathrm{N},\ \mathrm{A}\,\tfrac{1}{2}\,\tfrac{1}{2}\,\tfrac{1}{2}\,1\,1,\quad \mathrm{K}\,0\,0\,\tfrac{1}{2}\,\tfrac{1}{2}\,\tfrac{1}{2},\quad \mathrm{C}\,1\,1\,1\,1\,\tfrac{1}{2}$$

satisfy all of the above requirements except the validity of (vi). The requirement that $b \neq 1$ in C $b\,d\,e\,f\,h$, is not satisfied either. The set

$$(\gamma)\ \mathrm{N},\ \mathrm{A}\,0\,0\,0\,1\,1,\quad \mathrm{K}\,\tfrac{1}{2}\,\tfrac{1}{2}\,\tfrac{1}{2}\,\tfrac{1}{2}\,\tfrac{1}{2},\quad \mathrm{C}\,\tfrac{1}{2}\,1\,1\,1\,\tfrac{1}{2}$$

in which $b \neq 1$ in C $b\,d\,e\,f\,h$, satisfies all of the above requirements except the validity of (iv) and (vi). With this set, however, conjunction is not distributive with respect to disjunction, and conjunction and disjunction are not de Morgan transforms of each other.

We have shown that it is possible to define connectives for three values in such a way that the following statements are true when S and Q are statements which may be true, false, or meaningless, and O, O_1, O_2, O_3 are observation statements that are either true or false:

(vii) For any statements S, the observation statement O follows from S and S $\supset$ O without following from S alone.

(viii) For any statements S and Q, if S follows logically from O, then S $\vee$ Q, being a consequence of S, is also a consequence of O.

(ix) For any statement S, $\overline{O_1}\, O_2 \vee O_3\, \overline{S}$, with O_1, entails O_3.

4 We will now reassess the arguments in which these statements were made.

4.1 If (i) is valid, then it comes out true for all interpretations of its letters (e.g. $p = \frac{1}{2}$, $q = 1$). So although O follows from S and S $\supset$ O, from the fact that O, being an observation statement, is either true or false, it does not follow that S is not meaningless. Thus the argument in 2.1 shows, not that the principle, formulated in one way, allows meaning to any statement whatever, but that this formulation of the principle is not an adequate criterion of significance, since it is satisfied by statements that are meaningless.

If (i) is invalid, then (vii) is false, and the argument in which it is a premise is not conclusive. Thus whether (i) is valid or not, the argument 2.1 does not prove what it was taken to prove.

4.2 According to the criterion mentioned in 2.2, if a statement P is a logical consequence of S, then if S is a meaningful statement, P is also meaningful. This is the case only when $b \neq 1$ and $h \neq 1$ in C $b\,d\,e\,f\,h$, (γ) being an example of a set of definitions of this type which secures the validity of (ii) and (iii). If such definitions are adopted, then although it is true that if S is a meaningful statement, then S $\vee$ Q is meaningful, even when Q is meaningless, this is harmless. It is harmless because the definitions adopted are such that Q, being meaningless, is a logical consequence only of other meaningless statements, none of which can be a premise in an argument.

If (α) or (β) ($b = 1$, $h \neq 1$ in C $b\,d\,e\,f\,h$) are adopted, then S $\vee$ Q is a true statement if S is true and Q meaningless, and S $\vee$ Q is a meaningless statement if S is false and Q meaningless. In the first case, Q, being meaningless, cannot be deduced from true premises. In the second, although S Q is a logical consequence of S, since a false antecedent cannot be asserted, the meaningless consequent cannot be detached.

It appears that there is no reason why S $\vee$ Q should be ruled out as meaningless when S is meaningful and Q is not, but there are, as Hempel points out, other objections to the criterion that he is considering.

4.3 We have already shown that it is possible to accept both (1) and (2) in 2.3. We are now in a position to show that they are independent of each other.

To adopt the set of definitions (γ) is to reject (1) and accept (2).

To adopt the definitions (α) or (β) is to reject both (1) and (2).

Suppose that we adopt (1) and, despite the complications noted in section 3, give up (R), accepting as valid any schema which does not come out false for any interpretation of its letters. The schema $p \supset . p \vee q$ is valid, and $p \vee q = \frac{1}{2}$ when $p = 1$ and $q = \frac{1}{2}$. So P $\vee$ Q is a logical consequence of the statement P, even when P is a meaningful statement and $P \vee Q$ a meaningless one. Thus although (1) is adopted, (2) has been rejected.

4.4 If we adopt definitions (α) or (β), then (iv) and (v) are valid, and Church's argument does show of any statement that either it or its negation is verifiable. But since (iv) is valid, it comes out true for all interpretations of its letters, and although if $\overline{O}_1\ O_2 \vee O_3\ \overline{S}$ does not entail O_2, then S is indirectly verifiable, S may still be meaningless. If, on the other hand, $\overline{O}_1\ O_2 \vee O_3\ \overline{S}$ and thus $O_3\ \overline{S}$ does entail O_2, then $\overline{S}$ is directly verifiable. But in this case too $\overline{S}$ may be meaningless for if $\overline{S}$ is $\overline{O}_3\ O_1 \vee O_2\ \overline{Q}$, then $O_3\ \overline{S}$ entails O_2, and if O_1, O_2, O_3 are true and Q meaningless, then $\overline{S}$ is meaningless.

If we define connective in such a way that if $O_3\ \overline{S}$ entails O_2 then $\overline{S}$ cannot be meaningless, then (iv) is not valid. This is proved as follows:

Let $X\ Y \supset Z$ be a valid schema

It is required that

$$\text{if } X \neq \tfrac{1}{2} \text{ and } Z \neq \tfrac{1}{2} \text{ then } Y \neq \tfrac{1}{2}$$
$$\text{i.e. if } X \neq \tfrac{1}{2} \text{ and } Y = \tfrac{1}{2} \text{ then } Z = \tfrac{1}{2}$$

The only definitions that satisfy this requirement, and are extensions of definitions in the two-valued calculus are those in which

$$b = h = \tfrac{1}{2} \quad \text{in K } b\,d\,e\,f\,h \tag{1}$$
$$d \neq 1, f \neq 1 \text{ in C } b\,d\,e\,f\,h \tag{2}$$

Let negation be N

In (iv) it is required that

$$\text{if } p \neq \tfrac{1}{2} \text{ and } r \neq \tfrac{1}{2} \text{ then } \bar{p}\,q \vee r\,\bar{s} \neq \tfrac{1}{2}$$

Because of (1) and (2) this can be secured for all values of q and s only if

$$b \neq \tfrac{1}{2},\ d \neq \tfrac{1}{2},\ e \neq \tfrac{1}{2},\ f \neq \tfrac{1}{2},\ h \neq \tfrac{1}{2}, \text{ in A } b\,d\,e\,f\,h \tag{3}$$

If in (iv) $p = \tfrac{1}{2}$ and $r \neq \tfrac{1}{2}$

Since from (3) $\bar{p}\,q \vee r\,\bar{s} \neq \tfrac{1}{2}$

then by (1) $p \,.\, \bar{p}\,q \vee r\,\bar{s} = \tfrac{1}{2}$

But if $p \,.\, \bar{p}\,q \vee r\,\bar{s} = \tfrac{1}{2}$ and $r \neq \tfrac{1}{2}$

then by (2) $p \,.\, \bar{p}\,q \vee r\,\bar{s} \,.\!\supset r \neq 1$

i.e. (iv) is not valid.

If (iv) is not valid then (ix), which is one of Church's premises, is not true, and if (v) is invalid, then his argument is invalid. In either case the argument against Ayer's recursive criterion fails. If, on the other hand, we adopt definitions such that (iv) and (v) are valid, then it follows by Church's argument, that any statement is such that either it or its negation is verifiable, and by the arguments given above that a verifiable statement may be meaningless. In this case the recursive criterion stated in 2.4 is shown to be inadequate.

5 An attempt to formulate a general distinction between meaningful statements and meaningless ones is pointless unless some statements are meaningful and others not. If we accept that statements may be meaningless as well as true or false, then we must extend our notions of implication and the other logical relations to take account of this. Whether a particular argument is effective against a given criterion of significance or not, depends upon the way in which we extend these notions. The number of ways in which we might do this is very large, and an examination of arguments will not help us to choose between them, because our arguments normally proceed upon the presupposition that our assertions are either true or false. We can choose our definitions of disjunction, conjunction and the material conditional as applied to statements that are true, false or meaningless, only after we have decided which arguments are to remain valid and which not. If for example, we wished to retain the criterion in 2.4, we would define connectives in a way that would make Church's argument invalid. This would prevent confusions of the kind discussed in section 2, but it would do nothing to settle disagreements about meaningfulness, for if someone accepted as meaningful a statement meaningless by our criterion, he would reject the criterion, and would then have no reason to accept the definitions.

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REVIEWS

THE SCIENCES AND DEITY

I *Introduction*

MAN (under God) had for long been conceived as lord of the universe. But with the advance of modern science in all its branches, man has been dethroned. He has shrunk in significance. Copernicus, Darwin, Marx, and Freud, in particular, have more than ' taken him down a peg or two '. It is good of course in that many appallingly naïve superstitions have been swept away and a race of ' sensible ' humanists comes into being but they are depressed by the materialism that seems to be the natural result of the apparent unimportance of man. They must have a god somewhere, somehow, so that they can have Something to *look up to.* Luckily there seems to be a solution. Plain ' evolution ' won't do; but what about evolution-with-a-purpose, the purpose being the creation of consciousness, nay even self-consciousness, and man himself being in control of the direction and trend of the higher stages of this self-consciousness? If the universe is really to be viewed in this way then such a view is obviously not materialistic. Man is not an insignificant speck of dust. Deity there is—of a kind at least, even if perhaps amorphous and vague, and even if the relationship between deity and humanity is difficult to define satisfactorily (especially if the former is not yet fully ' there ' !).

During the last forty years a number of philosopher-scientists have tried to develop theories on this sort of general line. The latest is the French Jesuit priest Pierre Teilhard de Chardin (who was also a biologist and palaeontologist of some fame) who sets forth his views on this theme in *The Phenomenon of Man.*[1]

This book has been widely reviewed and widely discussed since its publication. All are agreed that it is tough reading,[2] in fact *a difficult and obscure book.* This is due perhaps partly to the style in which it is written. It is often ' poetical ' or ' mystical ' (i.e. life emerging from matter ' is dripping with molecularity ' (p. 92), or ' All round us one by one like a continual exhalation, " souls " break away, carrying upwards their incommunicable load of consciousness ' (! !) p. 272)) and this seems hardly excusable when we learn that the author opens his Preface thus: ' If this book is to be properly understood, it must be read not as a work on metaphysics, still less as a sort of theological essay, but purely and simply as a scientific treatise'. But indeed it is difficult to see how we *could* read it purely and simply as science, for it contains also philosophy and theology. One result of this is that even competent readers are far from being unanimous in their endeavours to explain what the book is about. Some have lauded it to the skies; others have dismissed it as utter charlatanism. But in any event the book is *important* if only because of the great interest it has aroused in quite large sections of the thoughtful public. Should the work be basically pseudo, it is *important* that its fallaciousness should be exposed.

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[1] Pierre Teilhard de Chardin, *The Phenomenon of Man* (translated by Bernard Wall), with an Introduction by Sir Julian Huxley. Collins, London, 1959. Pp. 320. 25s.

[2] The present reviewer must confess that had it not been for Sir Julian's so lucid ' Introduction ', he would have found the ' tough reading ' still tougher!

2 *Outline of Teilhard's Theme*

Mankind in all its aspects is a phenomenon to be investigated scientifically in the same ways as all other phenomena, so that human history like all other history and all human values are proper objects for methodical study. Man is thus not to be regarded as standing outside nature. It is not enough to conceive of him as merely evolved *out of* nature. His is an evolution which always, and all the way through, presents him as united *in* nature. He and the whole universe of which he is part-and-parcel is one enormous process of *becoming*, and in its development reaching higher and higher reaches of being, organisation, and complexity.

Teilhard recognises two different kinds of energy, (1) tangential and (2) radial. The former is more elementary; it is mechanical. Any increase in this form of energy retains its unity as a mere addition of components so that any aggregate reached remains at the same level of organisation as the separate components. But (2) radial energy, in any accumulation of its components, rises to more advanced levels of organisation and complexity so that its unifying centre becomes ever 'higher' in relation to the basic level of its components. The evolution of nature, from elementary particles culminating at last in the manifestation of consciousness is by the continuous growth of radial energy, thus we have a non-stop process of 'becoming'.

All this is a complete departure from the Aristotelian tradition of a 'fixed' universe. In fact Teilhard tells us, more than once, that we ought not any more to use the term 'cosmology' but rather *cosmogenesis*. Things cannot be classified any longer into a limited number of fairly clearly defined species. Instead of a steady-state universe Teilhard presents us with one that in all its parts is continually striving to be something different (and higher) at every moment.[1]

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[1] It is worth reminding ourselves however that at least this part of the Abbé's doctrine (this anti-Aristotelian conception), in essentials, is not *new*. Others have said much the same thing, notably Sir Charles Sherrington in *Man on his nature* (Cambridge 1940). And indeed there were elementary glimmerings in Heraclitus—500 B.C.

One very important thought in Teilhard's treatment of this evolution or becoming is that nothing at any stage of development, not even in the inorganic, comes (as it were) as ' a bolt out of the blue '. Everything has always already existed at an earlier stage if only in an obscure and primordial way. Love, for example, is not a new phenomenon coming to birth with the arrival of man. We can find a kind of premonition of love even in the presentient world (even the inorganic) in the propensity of molecules to unite; for example, sodium chloride is born out of the love of sodium for chlorine! So you cannot say ' life began here ', or ' at this point ', for life was there from the start. And even ' from the start ' is a false expression for there was not anything that could properly be called a ' start '.

So to describe man properly we must go back in history, to his origins, nay even to his pre-origins, and even here we have to look for obscure and primordial precursors (*ad infinitum*).

But this is not all. We have to evaluate forwards as well as backwards, the future as well as the past. Man, as well as other phenomena, can not be evaluated or satisfactorily described simply in terms of his origin; he must be viewed also in the light of his evolving direction, his inherent possibilities and limitations, and his calculable future trends. Thus total man involves a complex space-time relationship. He is what-he-has-been and what-he-is-becoming in relation to a ' becoming ' universe of which he is the present apex. When man evolves far enough by reaching higher levels of mental, moral, and social organisation, through the build-up of radial energy, Teilhard implies that a new word will have to be invented (superman?). The gradual ascent of man from the brute to higher and higher levels of humanity is called by the author, a process of *hominisation*.

Complexification. How does this great chain of evolution operate? By means of an ever-increasing complexity. Teilhard calls this whole process *complexification*. Cosmogenesis involves increasingly elaborate *organisation* which in fact we find as we pass from subatoms to atoms, from atoms to inorganic molecules and then on to organic (molecules), to sub-cellular living units, then to cells, many-celled individuals, cephalised metazoa with brains, then to primitive man and finally to civilised (and eventually to super-civilised!) societies.

But in addition to this (simple!?) form of *complexification*, there also occurs at all evolutionary levels a tendency towards *self-complexification*—this seems to mean tendency to higher degrees of the organic accompanied by more and more self-reference (i.e. *enroulement organique sur soi-même*). Thus in human beings (because of this tendency) evolution is apt to produce more and more mental convergence going hand-in-hand with a growth in self-consciousness.

Divergence and convergence. But this reference to *convergence*, and to *self-consciousness*, is perhaps to anticipate unduly. Let us go back, take cosmic evolution in its broad span, and its totality, and ask whether (according to Teilhard), and in what way, evolution makes for convergence, divergence, or both?

Through a period of some two thousand million years up to the advent of man evolution has been *genetical* having a natural tendency to produce separation and division; by a sort of centrifugal differentiation the cosmos has developed into more and more fragmentation. But with the advent of man evolution becomes convergent, through the common mental activity of humanity mediating through social, cultural, spiritual and aesthetic movements, there develops an overwhelming progress towards ever greater *unity*. This is a new style of evolution (pp. 166 and 286), quite different in its nature from the genetic. It is a sort of *psychosocial* evolution made possible by the fact that man, and man alone, is the only species that has developed a reflective consciousness—one capable of thinking of *itself as also being a phenomenon* among other phenomena. The development of self-consciousness tends more and more to superimpose centripetal or centrifugal tendencies. So the long history of cosmogenesis and hominisation with its divergence followed by convergence may be illustrated perhaps by the meridians on a globe that begin by diverging as they leave the Pole but which approach one another again after the Equator . . . (p. 272).

The parallel of course must not be understood too exactly. It is not literally accurate, for the psychosocial evolution of man is not solely centripetal or convergent. Teilhard fully recognises that as civilisation develops there are divergent trends within the total sphere of the convergent. There is, for example, cultural differentiation, that is the production of numbers of psychosocial units with different cultures. But these are not strong enough or lasting enough to disintegrate the predominating urge towards unity. Cultural diffusion, fostered by migration and improved communications, tends to lead in time to cultural convergence.

As this evolution towards greater convergence develops through psychosocial media, the mental activity of mankind becomes more and more complex. This mental activity, or sphere of mind, the author calls the *noosphere*. All the separate parts of this noosphere, as a result of easier world communications and the increase in populations, are being gradually fused together, so that there is an increase of tension and complexity of organisation. This kind of feverishly congested psychological, or psychosocial, activity is pushing (or perhaps ' leading '?) humanity to ever higher phases of hominisation.

These higher phases Teilhard calls ultra-hominisation—the gradual growth of mankind into one all-embracing psychosocial unit. Evolution has already advanced so far in the way of producing a sort of unifying centre to mankind's system of thought that this may reasonably, he thinks, be conceived of as a ' head ' (a sort of cephalisation of society). This he sees as an analogy with the literal and physical fact that the human (and other) organisms have evolved heads as the centrally directing part of the body. Teilhard believes that, as psychosocial evolution advances, this head of human society will come to recognise itself and understand itself more clearly, and that thus our descendants will be enabled to plan and control the future trend(s) of their evolution more completely.

Omega Point. But the Abbé has even more than this in mind. The end is what? Even that higher phase of evolution, self-determined hominisation, is not an end in itself. It must (apparently inevitably?) lead to *point Omega.* The purpose of evolution is the production of consciousness in ever higher and more intense form. Not only hominisation but all cosmogenesis is directed to that end. Point Omega is the final state of all this gigantic becoming—it is a kind of super-consciousness, or rather Super-selfconsciousness. The main argument for our belief in the reality of Omega is simply that since life, and even consciousness, has had precursors or ' premonitions ' in the past, even at elementary levels, the very law of this evolution compels us to believe in the attainment of higher levels of consciousness in the future.

Two main things are contributing to the achievement of Omega, according to the Abbé. The first is our growing knowledge about the Universe generally (including outer space, and also humanity both as individuals and as society). The second is the intensification of feverish psychosocial energy on a planet of limited surface thus making for more and more unification in human life and thought (pp. 250–260).

What is Omega point? Teilhard says[1] it is a distinct centre radiating at the core of a system of centres, and that it is a Soul of souls, and also that it is autonomous, actual, irreversible and transcendent. From one point of view Omega is *human society* the individual members of which have become so united in mind and heart that they have, as it were, got a single intellect and will and set of attitudes between them all. It is human society having evolved into a Hyper-Person.

From another point of view Omega is simply God or Deity. But in some way that Teilhard does not make clear, God and Society are quite distinct. God is not identical with humanity. There is not pantheism (p. 309). In this omega point all man's conflicts are resolved in God, for God is the reconciling principle of Unity. And yet God is transcendent; humanity is not simply swamped by, and merged in, God. At the Omega Point, he says we find ' the Future-Universal '. Future! And yet it is also ' in some sort loving and lovable *at this very moment* '!

But Teilhard is not content to assert that Cosmogenesis and Hominisation (which are essentially what the universe is) involve belief in some form of theism. It is according to the Abbé specifically Christian. That is to say the God of the Universe is specifically and necessarily the God of the Christians. 'God (is) the Centre of centres. In that final vision the Christian dogma culminates' (p. 294). And again,

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[1] On pp. 262, 268, and 271.

> To live and develop the Christian outlook needs an atmosphere of greatness and of connecting links. The bigger the world becomes and the more organic become its internal connections, the more will the perspectives of the Incarnation triumph. That is what believers are beginning, much to their surprise, to find out. Though frightened for a moment by evolution, the Christian now perceives that what it offers him is nothing but a magnificent means of feeling more at one with God and of giving himself more to him. In a pluralistic and static Nature, the universal domination of Christ could, strictly speaking, still be regarded as an extrinsic and super-imposed power. In a spiritually converging world this 'Christic' energy acquires an urgency and intensity of another order altogether. If the world is convergent and if Christ occupies its centre, then the Christogenesis of St. Paul and St. John is nothing else and nothing less than the extension, both awaited and unhoped for, of that noogenesis in which cosmogenesis—as regards our experience—culminates. Christ invests himself organically with the very majesty of his creation. And it is in no way metaphorical to say that man finds himself capable of experiencing and discovering his God in the whole length, breadth and depth of the world in movement. To be able to say literally to God that one loves him, not only with all one's body, all one's heart and all one's soul, but with every fibre of the unifying universe . . . that is a prayer that can only be made in space-time (pp. 296-297).

So the progress of the sciences reveals a Christogenesis. Christ is becoming, but also He *is*, and He *was*; so mankind is reassured.

(I can only hope that the above has been, on the whole, a fair presentation of Teilhard's main thoughts. But it must now be examined in order to discover how valid, or otherwise, it may be.)

3 *Critical Evaluation*

In trying to make a critical assessment of this book, it is desirable to divide it into two parts. (*a*) The first and larger part (but doubtless not in the author's view the more important) is, in the main, straightforwardly scientific and factual, being largely palaeontological and biological. (*b*) The later chapters are much less empirical and far more 'mystical' and speculative, relying considerably on theological, and quasi-theological concepts, and these tend inevitably to be suspect from the point of view of strict science as being usually somewhat gratuitous.

Let us start with (*a*) (above). The general thesis that the universe, with man at its apex, is evolving to higher and higher stages of development and organisation, that man is unified *in* nature as a phenomenon sharing in the common being of all other phenomena, that evolution manifests both divergent and convergent traits, and that man can only be properly evaluated in the light of his future development as well as in the light of his past—all this seems scientifically respectable, and few educated people these days would wish to dispute it.

And indeed further, when he claims that evolution, with the appearance of man and his power of *reflection*, has entered into ' an entirely new field of evolution ' (p. 286) (what earlier in this article I have called *psychosocial* evolution), he is simply saying something with which most scientists would agree. The thought is momentous and valuable.

But when we are told (pp. 243 and 258) that evolution is simply equivalent to *rise of consciousness*, and even increased consciousness, one cannot but wonder whether Teilhard is not shutting his eyes to some inconvenient facts. Admittedly evolution *generally* tends towards consciousness and more consciousness. But not always. Bony fishes and birds have not developed cerebralisation and yet they are successful lines of evolution, i.e. after the lapse of a relatively long period of time these species continue to exist and to flourish. Thus the *unity* of the universe (which Teilhard so earnestly seeks) is impaired.

And again, believing that evolution is *essentially* leading to consciousness, he stresses that cosmogenesis has a definite *direction*. But has it? Most lines of evolution lead to the complexification of the nervous system (necessary for consciousness), but some do not.

(*b*) *The mystical and speculative parts of the book—being in the main concerned with ' point Omega '*. If we look upon Omega as a general conception according to which more perfect *hominisation* looms on the *possible* horizon of man's future, it may seem reasonable enough and worth our belief. But Teilhard comprehends much more than this. Bearing in mind that he writes more often than not of ' Omega Point ' and not just ' Omega ', we must ask the following pertinent questions:

(1) Have we good and sufficient reason to believe in the *reality*[1] of Omega point?

(2) Is Omega point (Higher Consciousness, God) which is (being?) produced by Evolution consistent with the Christian concept of God?

(3) Is Teilhard consistent with himself in the account which he gives of the nature of Omega Point?

(4) What is the relationship between Teilhard's God and the Ultimate Hyper-personal Society, and does such relationship seem reasonable?

(1) *Is Omega logically demonstrated?* Teilhard has clearly warned us that his book is to be read ' purely and simply as a scientific treatise ' (p. 29). Also, he has stressed that he arrived at his Omega point by a remorseless logic. In fact this concept is not scientifically demonstrated at all. It is

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[1] i.e. not as Something that may be *possible* in time, but as Something actual and even APODEICTIC.

simply asserted. 'To be supremely attractive, Omega must be supremely present'! Of what kind is the argument here? At least it is not the argument of science. And indeed does he not betray the wishful thinker and 'give the show away' when he writes '. . . *doubtless I should never have ventured to envisage the latter* (i.e. Omega Point) *or formulate the hypothesis rationally if, in my consciousness as a believer* (i.e. in the Christian dogma), *I had not found not only its speculative model but also its living reality*' (p. 294, italics mine).

And if the sciences, on their own account and unaided by Faith, were unable to afford this scholar with his undoubtedly scientific mind good and sufficient reason to believe in the reality of Omega Point, are his readers likely to find that same *a priori* Faith unnecessary in order to accept the reality of Omega? Surely not.

(2) *Is Omega consistent with the Christian's God?* In his exposition of Omega Point, Teilhard clearly seems to have allowed (surely he must have perceived it?) a confusion of thought between God-in-the-past and God-in-the-Future, between the Christian God and an Emergent Deity. In fact, Teilhard by making no explicit recognition of the truth that these two conceptions (at least *prima facie*) are the contrary, the one of the other, gives the impression that they are one and the same. It is as if (for the Abbé) the concept of Deity could not be *in any sense* related to the *time series.* But this surely needs searching examination? The question might be asked thus:

Is God (*a*) *before*, or is God (*b*) *after*, Evolution? (And if both, How so?)

(*a*) Traditional (Orthodox) Christianity depends basically on the assumption that God, perfect and fully-existing, was 'there' prior to (BEFORE) the beginning of things.

(*b*) But (as opposed to this) lately the Emergent Evolutionists (Samuel Alexander and the like)[1] have gone to the opposite extreme, and have 'preached' God as emerging—as striving towards fully realised existence only in the higher reaches of mind-in-evolution. According to this view God is (as it were) AFTER evolution.

Now Teilhard—who regards his book as primarily a kind of Vindication of Christianity—where does he stand in this polarity? Is his Omega point in line with (*a*) (above)? The answer is clearly that much of his treatment of Omega is quite *inconsistent* with the God of orthodox Christianity. For example (on pp. 268-9) he says 'the idea that some Soul of souls should be developing at the summit of the world is not as strange as might be thought . . .' *Developing* indeed! (Is not that an *Emergent* Deity . . . or near enough?) And there are other passages (pp. 259-60) that strongly suggest that Teilhard's Deity is still only evolving-towards-Himself! Anything but Christian!

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[1] Teilhard does not mention any of them in his book, not Alexander or Lloyd Morgan, or even Bergson (who had definitely influenced him in his youth). But unquestionably much of the so-called 'Emergent Evolution' as understood in the 'twenties is easily recognisable in this book. Dare one ask—'can it possibly have been that Alexander and Company had more " nerve " than Teilhard, that they did not need the comfort of Christian myths?'

(3) *Is Teilhard's account of Omega self-consistent?* But it is apparent that the inconsistency just noted above between Omega and the Christian Deity falls back upon something deeper, namely, a fundamental inconsistency in Teilhard's treatment of Omega itself. He surely contradicts himself? For instead of writing consistently of Omega which is (in process of) coming-into-being, he says on page 291 of his book (as if creating a bulwark for Orthodox Christianity) 'Omega is . . . *already in existence*'. (And, incidently, one may wonder why the italics?).

Does it not seem as if neither science nor Orthodox Christianity could be satisfied with this book, as it stands? The Deity has not been shown to be 'Christian-And-Emergent', nor yet 'Either Christian or Emergent'. There is no Deity at all. He 'falls between two stools'.[1]

(4) *What is the relationship between Teilhard's Deity and the Ultimate Hyper-personal Society*? We are evolving (all of us together) into an ultimate hyper-personal society; 'through science and the philosophies' we advance to 'a collective human *Weltanschauung*' and in this we have the first symptoms . . . of a still higher order . . . somewhere ahead a point which we might call Omega . . .' (p. 259). This looks like pantheism. But Teilhard strongly refutes such a notion for, he says, this 'universal centre of unification . . . must be conceived as pre-existing and transcendent' (p. 309). Or, he says, call it 'pantheism' if you like but it is 'an absolutely legitimate pantheism' (p. 310).

But surely, and at best, these are only 'pious' assertions that contradictions are resolved, not *demonstrations* as to how we can satisfactorily conceive of the relationship between an Omega that is a Super-social Organism and an Omega that is a pre-existing transcendent Deity?

4 *Summing-up*

To sum up. *The Phenomenon of Man* can lay claim to a modicum of importance and is sound enough in so far as it does supply a considerable amount of concrete (scientific) evidence for the belief that life in general, and man in particular, are evolving towards higher levels of being which may be more and more realised in the future.

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[1] Nevertheless it must be admitted in fairness to Teilhard that it is only from the point of view of *traditional modes of thought* that he can be indicted for a *prima facie* inconsistency, for sometimes writing as if God were only in process of emerging, and at others as if God (as the Bible teaches) was in full possession of his Deity at the beginning. For it seems just *possible*, if one could rise to a plane of thought based on some *higher synthesis* (as, for example, by the application of Einstein's Relativity principles) that the idea of Perfect Deity gradually-evolving is still consistent with Perfect-Deity-at-the-beginning as the presupposition of the Universe, but *prima facie*, so ridiculous is this that it certainly cannot be taken for granted, or the problem of reconciliation ignored, as it seems to be in the book under review.

In general terms, the book is a reassertion of belief in human (as well as cosmic) progress so dear (though of narrower interpretation) to the nineteenth century, and so suspect and unfashionable, in many quarters, in the twentieth. Belief in man. This is a tonic that mankind sorely needs in these days of spiritual doubt and social change. And when belief is based on the demonstrations of scientists rather than on the wistful yearnings of uncritical prelates (as has been the lot of mankind till quite recently), then there is sound hope for man's future.

And in this note of optimism and positivism, happily for our age, this work does not stand alone. It is in line with the thoughtful optimism of Professor Michael Polanyi's ' Beyond Nihilism ' (Eddington Memorial Lecture 1960).

But it is difficult to see how one can go further along with Teilhard, how one can accept his contention that his ' Future Universal ' (Omega) which the sciences do indeed seem to be *presaging*[1] is to be equated with the traditional transcendent God of Religion and (as if this was not asking enough of the sciences) even of vindicating the Christian Faith. Here there is surely a saltus, a *Deus ex machina*.

If this criticism is valid, then the book under review illustrates very well how greatly the tradition of much Western philosophy has been vitiated by the tendentious thinking of theologians, or at least by theologically-minded philosophers; vitiated by men who, nurtured in the first place by some fortuitous set of religious dogmas, set about (consciously or subconsciously) to rationalise them, to find a theory of the universe which would vindicate a specific[2] religious tradition. Let us hope the universe may be ' proved ' teleological, let us even hope (if we want to) that Deity, in some sense, may be *proved*, but in the name of truth let all our philosophy be scientific (i.e. objective, unprejudiced, disinterested, the comprehensive product of cosmopolitan mind) and not the result of our finding fortuitous reasons for what we believe because of our personal instincts, education, or particular cultural tradition. (The universe is too big and too complex for such petty-minded philosophising).

Finally, the fact that *The Phenomenon of Man* has been more of a 'hit' than it deserves, that it has received exaggerated acclaim from so many Critics is easily explained. There is in many quarters today a climate of dread uncertainty in matters of religious faith, a spiritual loss of nerve, a longing to be reassured, almost at any price, by an accredited Authority, that the Sciences have not in fact banished God from the universe. Here is a book from a Priest who is *also* an *eminent scientist* (a rare combination!). The strain is over. Men are only too ready to allow sober judgment to lapse. God is saved. We can 'let down our hair' and heave an almost hysterical sigh of relief.

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[1] The Abbé would no doubt reject ' presaging ' here as not being strong enough. His word (in English), one imagines, would be ' proving '.

[2] This is not only exemplified in the ' Scholastic ' philosophers. We find it too in Hegel, and in this country in most of our Gifford Lecturers even in the past few decades.

And, notwithstanding all this, we still like to think of ourselves as adult.

Alan L. Stuart

An Introduction to the Logic of the Sciences. By R. Harré.
Macmillan, London, 1960. Pp. viii + 180. 21s.

The central part of this book seems to be the discussion of the nature of explanation. An explanation is regarded as satisfactory only if in addition to being in accordance with the customary formal requirements it also provides understanding (32; 104). Understanding 'is gained either by our finding an illuminating analogy to the phenomenon whose character we do not understand, or by our " exposing a hidden mechanism " the workings of which invariably result in the phenomenon that required explanation' (82). The difference between an empirical law which correlates, and condenses observational facts without providing a reason for the correlations on the one side and a law which gives a rational account of natural phenomena on the other lies just in this: simple generalisation will not provide understanding (41 f). 'The extra that is required is an appropriate model, description of which, linked to the description of the facts the theory is intended to cover provides . . . the basis for scientific explanation' (143). Apart from providing such understanding a model, or a well-used analogy, also contributes to the further development and extension of the theoretical framework already in existence (97 f) and it may finally lead to 'explanations of such generality and power that at least within certain restricted classes of phenomena nothing is left unexplained and everything can be foretold' (42). It is not necessary that the model be immediately accessible to test (150 ff). Indeed, as soon as testing has made it accessible to empirical inspection, in the very same moment it loses its capability to explain and turns into a fact which is now itself in need of explanation (100). Taking all this into account we may guess that the author is concerned (although, unfortunately, not in a very explicit manner) with the rôle metaphysical ideas play in the process of explanation and theory-construction. For him such ideas are not only psychological devices which may cause the production of important laws. They also play an essential rôle in the *arguments* which lead up to such laws and theories as well as in the conditions for a satisfactory explanation.

Before discussing the possible applications of this interesting and fruitful point of view it would seem to be advisable to free it from a restriction that is not really necessary. The restriction is that the model, or the analogue used is required to be familiar (32) and not purely formal (159). Considering that the author makes understanding a necessary criterion of a satisfactory explanation this restriction would seem to be quite plausible. However it is well known that intuitive plausibility is very often the result rather than the presupposition of the continued application to concrete problems. It is therefore not advisable to exclude a model, an analogy, or a more general idea on account of its initial unfamiliarity. A second criticism is much more important. The criticism is that the author has failed fully to utilise the power of his theory of explanation. He has not applied it to other items dealt with in his book and he has thereby missed the opportunity of giving the book a more coherent appearance. Thus his discussion of the peculiarities of scientific description would have greatly gained from relating it to the context of explanation and theory-construction. First of all the notion of an observable could have been made much clearer in this way. Observables are introduced as elements of the smallest possible set of predicates capable of specifying all the properties of a system (48). This definition produces a well defined set of observables only if the set of properties of the system investigated is also well defined. The only way of obtaining a well defined set of properties is by appeal to the theories we possess. But if we are allowed to appeal to them, then we may as well define the notion of an observable in a more direct way, namely as referring to all the primitive descriptive terms occurring in these theories. Secondly, the syntactical means for forming scientific descriptions are of course also dependent on the theories and should be explained in connection with them. (By the way I do not think it to be the task of a philosopher of science to popularise such notions as the notion of a function, of a functional and other mathematical notions; this applies even to introductory courses such as the one under review. My advice would be either completely to omit technicalities—and there is quite a lot to be discussed in a non-technical manner—or to assume them as known. Too much of contemporary philosophy of science is nothing but bad, because unnecessarily technical, popularisation!) Thirdly both the explication and the reduction of predicates (65) is understandable only if we discuss the conditions according to which theories in a certain domain are replaced by better theories in the same domain. For example the replacement of such qualitative predicates as pitch, loudness, timbre by quantitative predicates (frequency, amplitude, intensity of Fourier components) can be justified only by pointing out that the new predicates play an important rôle within theories which, judged by the standards of explanation, are more powerful, more satisfactory, more general than the (already very powerful) theories utilising the qualitative notions (the theory of harmony is an example). Hence also reduction and explication must be discussed in the context of explanation and becomes quite unintelligible when separated from this context. It should of course be mentioned, in all fairness to the author that this method of gradually proceeding from the less complex (concepts, statements) to the more complex (theories, sets of theories such as occur in explanations) is quite customary in contemporary discussion of scientific method. To a certain extent it occurs even in such an excellent treatise as Professor Nagel's recent *Structure of Science*. However, just as a concept can be made clear only from the function it plays in a statement—a fact that has been seen, among others by Wittgenstein who therefore in his *Tractatus* took sentences as the fundamental units of discourse—in the very same manner a sentence, its structure, the way it will be replaced, in the course of scientific progress, by a different sentence can be made clear only by pointing to the rôle it plays in the wider context of a theory, or of an explanation, that last and final unit of scientific knowledge. It seems to me that many of the more technical problems that at present beset philosophers of science (examples: What is a scientific law? How can the adoption of vacuously true statements be justified? What rôle do counterfactuals play in the sciences?) will be accessible to immediate solution once it is remembered that all scientific statements must be capable of functioning in explanations (otherwise they would be redundant) *and that all these statements are therefore subject to the conditions of explanation.* That is, the solution of the problems mentioned above will follow from these conditions and it will not of course be available to those philosophers who discuss the status of scientific laws and of scientific description independently of the nature of explanation.

Instead of relating statements to the context of scientific explanation where *reasons* are given for whatever rules (syntactical or semantical) are being used, many philosophers who feel the necessity of relating things to a wider context proceed *historically.* The same is done by our present author. He believes that ' if we want to understand the special sort of describing and explaining that scientists undertake we must first understand the way we ordinarily describe and explain events ' (4). The result of this belief is disastrous. Apart from giving rise to some quite childish analyses of the common idiom (cf. the turtle-dialogue on p. 9) it precipitates discussion reminiscent of the famous explanation of the principles of radio-telegraphy: *Question:* how does radio work? *Answer:* It is quite simple. Imagine a dog extending from Oxford to Cambridge. If you pinch the dog's tail in Oxford, he will naturally bark in Cambridge. Now radio is exactly like this, only without the dog. What I mean to say is that scientific explanation also in the sense of Harré contains so many modifications of what is regarded as a satisfactory explanation within the common idiom that any attempt to explain it with the help of the principles underlying the latter must sound like this little story. I really wish that writers in the beautiful and exciting field of the philosophy of science would stop appealing to the reason of ignorance even if appeal to such reason should happen to be very fashionable.

A similar remark applies to chapter V. This chapter contains some very clearly formulated criticisms of attempts to solve the so-called problem of induction. I especially liked the argument (which is by no means original, but still worth making, especially when it can be presented in a simple and concise form) that the probabilistic accounts of induction are not a bit better off than their less sophisticated predecessors (122). The ' method ' (125) of Strawson and Edwards, too, is rejected by a criticism which is as brief as it is sound and which to me at least seems to go to the heart of the matter (it deals with the *normative* aspects of scientific method (127)). But then the author starts wavering, repeats the now fashionable *bon mot* that scientific procedures ' form a spectrum ' (130) whose elements must be found out by ' a study, from a logical point of view, of the methods and criteria actually in use ' (129) and that it would be a ' fundamental misconception ' to believe that there is a standard way in which theories are abstracted out of factual evidence (129). Now it is of course quite correct to say that there exists a great variety of ways in which theories are first discovered. However if the author had read a little more carefully Professor Popper's book on the *Logic of Scientific Discovery* (to which he refers briefly on page 132 but whose views he regards as ' too extreme to account for many scientific activities ' (133)—as if Popper had intended to give such a *factual* account) he would have realised that this does not mean that there exist equally many solutions of the problem of induction. For example the fact that sometimes generalisations are not obtained by a stroke of genius but in a rather mechanical way (141) does not mean that induction *does* work in some cases. A closer inspection of the allegedly mechanical procedures will always reveal that what we are dealing with are in fact *deductions* from premises which have not been explicitly formulated and which have been obtained by a bold act of imagination in the first place. (This feature of theorising has misled other thinkers, too. An example are the various attempts to establish a ' Logic of Discovery ' including the most recent one by Professor Hanson.) The inductivistic bias of the author is shown also in other places; for example it is shown in his belief (153) that ' generalisations and theories are built up in such a way that they always stand a good chance of being confirmed, a better chance of confirmation than of disconfirmation ' and in the complementary remark that generalisations can hardly ever be shown to be completely wrong—unless ' we begin to apply the law to cases further and further removed from those cases upon which it was [originally] based ' (154). The trouble with such a principle of caution is that it is inapplicable unless we regard the law as nothing but a summary of past experiences. It is inapplicable because we hardly ever know (in advance of introducing new theories, that is) what cases are ' close ' enough to our initial evidence not to endanger the theory. Considering that Newton's theory evolved from the analysis of planetary motion one would be inclined to think that its application to double stars was very far removed indeed from the original evidence. However it was not through *these* cases that the theory was disconfirmed (or *could* be disconfirmed) but through the much ' closer ' case of Mercury. It would be unjust to omit mentioning those instances where the author has definitely abandoned inductivism such as when he says that scientists ' are often prepared to sacrifice something of precision and much of comprehensiveness if some system and order can be imported into a set of generalisations ' (72) or some metaphysical idea superimposed upon them (contrast this, however, with ' scientists are the misers of logic; they throw nothing away ' (157)). And it would be equally unjust to overlook the use he makes of his idea of explanation in chapters V (' Inductive Reasoning ') and VI (' Fitting the Facts '). But the lack of a clear conception of scientific method (saying that there is a ' spectrum ' (130) of procedures cannot be regarded as indication of such a clear conception as we know in advance that all sorts of things occur in any kind of activity!), the inconsistencies encountered in the book as well as the ballast of fashionable but otherwise irrelevant ideas it contains make it very difficult to recommend it as what it is supposed to be, namely, 'a short and systematic [*sic*!] account of the workings of the sciences for those students who are taking Logic or History of Science as subsidiary subjects ' (i). Whatever valuable elements the book contains could have been condensed (and as a matter of fact had already been condensed by the author (ii)) in an article or two. I really do not see any point in increasing the flood of books on the philosophy of science if twenty pages can do the trick.

I shall conclude with a few scattered remarks: (1) Considering the work of Duhem, A. Maier, Clagett, and others it is high time to stop criticising the Middle Ages for their alleged lack of scientific development (68). (2) It is also incorrect to say that science and the methods characteristic for this enterprise cannot start before a reasonable amount of empirical generalisations has been assembled (43). As has been shown by Popper and Matson and as had already been insinuated by Burnet theoretical physics starts with the Presocratics. It is statements like the one made by the author that have seduced people with insufficient historical information such as the late Professor Austin into thinking that philosophy can be advanced only in a piecemeal manner. (3) The author adopts the position of Mill, Ryle, and Toulmin according to which scientific laws are to be regarded as rules of deduction rather than as premisses (80 f.). Apart from some unenlightening verbal tricks the only argument the author can present in favour of this position is Carroll's infinite regress. That this argument is completely ineffective can be easily seen if we assume that all P-rules (and there will be a finite number of them) have already been turned into premisses. There not being any P-rules left the argument cannot even start. Hence, it cannot prevent this situation either in which all P-rules are used as premisses. (4) A heavy object hitting the sun would not be ' reflected violently back ' (144). It would be swallowed by the sun. This remark may seem pedantic but I believe that even a *philosopher* of science has some duty to use realistic examples in order to make the point he wants to make.

P. K. Feyerabend

The Edge of Objectivity. An Essay in the History of Scientific Ideas. By Charles Coulston Gillispie.
Princeton (Princeton University Press); London, Oxford University Press, 1960. Pp. viii + 562. 42s.

The Making of Modern Science. Six Essays. By Gerd Buchdahl, M. A. Hoskin, A. Rupert Hall, Marie Boas Hall, Sam Lilley and Charles Raven.
Leicester, Leicester University Press, 1960. Pp. 56. 6s.

Since the assumptions and conclusions of the philosophy of science are not completely divorced from the practice of science, philosophers of science have the problem: how to learn and understand the parts of science relevant to their enquiry? *Nature* and *The New Scientist* are not really adequate. If one is lucky, one may resuscitate memories of the Mathematical Tripos, and bring them into focus with the lenses of, say, Russell and Braithwaite. Or one may try a brain-picking session with a working scientist, but this produces frustration: the problems usually least interesting to him are those most interesting to the philosopher, and vice versa.

So the enquiring philosopher turns to the history of science. But new problems arise: which history? There is a growing awareness that the last word on the history of mechanics was not said by Mach. Even Koestler's *Sleepwalkers* has not received unanimous acclaim from the professionals. Worse yet, so much of the history of science has been done philosophically. The simple question, ' what was Galileo doing in his inclined plane experiment? ' has been answered, not once but too many times. Quite different answers have come from historians with a prior commitment to mathematical idealism, inductivism, refutationism, or dialectical materialism. At this point the philosopher may ask, ' Is there not a general account of, say, the Scientific Revolution, which is reliable but not pedantic, and neither naive nor axe-grinding in its philosophical commitment? ' In short, something he can use. It is with this question in mind that I discuss the two books under review. But first I should say that the situation is not desperate; the books on the Scientific Revolution by Wolf (revised by McKie), Butterfield, Crombie, and Hall all go some distance, each in its own way, to meeting the specifications set out above.

It would be hard to find a pair of books on roughly the same subject, with more differences between them. *The Making of Modern Science* is a solid, workmanlike job of popularisation, originally done for the BBC and now printed in this modest booklet. There is a certain unity about its theme, but the separate essays approach their material in different ways, and we have the vociferous outsider, Canon Raven, concluding the book by denying all that went before. *The Edge of Objectivity* is written by an American historian of science justly distinguished for his scholarship and insight. It is a big (and not inexpensive) book, devoted to exploiting one single theme, and doing it throughout with charm and elegance.

Since failure is more instructive than success, I shall concentrate first on Gillispie's book. What went wrong here? It would seem to be a result of his re-writing the history of modern science to prove a not entirely correct and not altogether clear point about the development of science: that it advances in each sphere by improving its ' objectivity '. Each dramatic step forward is interpreted as an advance in the ' cutting edge of objectivity '. There is, of course, *prima facie* evidence for this: the exorcism of spirits (of all sorts) from the natural world was one of the distinctive achievements of the scientific revolution. But ' objectivity ' is, without further definition, an omnibus word, and when it is used to describe everything from Kepler's *vis motrix* to Einstein's space-time continuum, it loses what precision it might have had originally.

Also, this cult of objectivity turns Gillispie's essay into Whig history with a vengeance. In this success story there is no mention of those who tried to advance ' objectivity ' before the time was ripe, ranging from the Mertonians of the fourteenth century, through the iatromechanists of the seventeenth, to the eugenicists of the nineteenth. One would not wish the history of science to turn into a morbid study of failures, but this succession of miraculous interventions by great ' objectifiers ' falsifies what Gillispie himself knows of the character of scientific progress.

Perhaps it was this straining after an impossible moral which caused Gillispie to make a distressingly large number of slips on historical fact. An erratum page lists some, and A. R. Hall's review in *Isis* (September 1959) gives a goodly number more. An example of a more serious error, missed by both these, is Gillispie's ascription of Fermat's ' minimum principle ' theory of refraction to Descartes. The difference was not one of detail: the Fermat theory yielded a reduced velocity of light in a refracting medium, and the Descartes theory an increased velocity. When an historian finds this sort of error appearing within his own field of competence, he is reluctant to rely on the book outside this field, and its value as a scholarly contribution is seriously reduced. Even on topics closer to his special interest, Gillispie makes unfortunate slips. Thus it is not correct to say:—' Diderot, ever the master of the philosophic fiction, created a blind mathematician, one Saunderson, who could conceive geometry to perfection. He had a post at Cambridge. But he had only a partial grasp of the real world . . . ' (p. 164). If this blind Saunderson had a creator, it was not Diderot, but *le bon Dieu*; he succeeded Whiston to the Lucasian Professorship in 1711, and was a well-known figure in English science (D. E. Smith, *History of Mathematics*, vol. I, p. 454, portrait p. 455).

One clue to the failure is in the Epilogue. There one turns, hoping for a drawing-together of the threads of the argument, and a clear statement of the message of the book, which up to then had been relegated to a few sentences at the end of each chapter. What one finds is a highly technical analysis of the different problems leading to Einstein's relativity theory: Mach and Maxwell are fitted into place, and marks for objectivity are awarded. But why is this an Epilogue, rather than a last chapter? And is this the end of the story? Since 1905 there have been a number of theoretical developments with philosophical interest, among them the interpretations of quantum mechanics, turning on this very question of objectivity. On this Gillispie has not a word to say.

Gillispie's failure to state his case cogently suggests that he had difficulties with the case itself. This lends support to my suspicion that this is not ' the book ' with which he has been living for some years, but a peculiarly muted version of it. (He mentions at the end that he found it impossible to work within the agreement with the publisher who first commissioned the book, and so transferred it to the Princeton University Press). For the moral of this present story relates only to the intellectual content of scientific explanation, and over the years Gillispie has shown himself to be on to a bigger, and more fundamental, problem. ' Objectivity ' is more than a successful tactic; it is a commitment without which science cannot survive. Let me quote a passage, starting (for brevity's sake) with the last line of a quotation from Galileo:

> But the conclusions of Natural Science are true and necessary, and the judgment of men has nothing to do with them.'
>
> To suppose otherwise is to give the game away. It is to agree that the papal court was justified when it decided to condemn Galileo on the ground that the Copernican system, however convenient as a mathematical device, tended to

> unsettle men's minds and weaken authority and order. It is to agree that the Soviet state was justified when it imposed Lysenkoism on the science of genetics as a theory conformable to Marxist dogma.[1]

Now, this is a challenging problem. I recommend it to philosophers. Had Gillispie written his book about that, we would have had something to argue about. But for reasons of his own, he did not, or perhaps could not. And on the problem which seemed historically more straightforward, and philosophically more fundamental, he foundered. It is tragic. Finally, advice to philosophers interested in science: a stimulating book, but *caveat lector.*

To turn from a brilliant failure to a modest success, we consider *The Making of Modern Science.* If shows that Gillispie's hope of winning for history of science 'a place in historiography comparable in interest and professionalism to that which the philosophy of science has long held in philosophy' is within sight of being realised. We now have a generation of historians who are prepared to recognise and grapple with the complexities of history, and not try to smother them under a single all-embracing philosophical blanket. Historians of different persuasions can now exchange the insights produced by their special viewpoints, and not merely indulge in polemics on the demerits of each others' grand syntheses.

The authors of these brief essays are not concerned to give a potted version of the technical history of science in the century of revolution. They presuppose an adequate knowledge of what actually happened, and consider a much more interesting problem. This is to give a sketch of the history of the concepts, methods and presuppositions of science, an investigation now called 'the history of scientific ideas'. This study is only now getting underway as a discipline, and is perhaps the most difficult of all sorts of histories to pursue. I think that it is a mark of the quality of these essays that the difficulties appear as clearly as they do.

The first two deal with more straightforward problems, and are correspondingly easier. Gerd Buchdahl reviews the explicitly formulated philosophies of science of Bacon and Descartes, and shows clearly that the difference between them was by no means one of hack inductivism against arrogant apriorism. Michael Hoskin, on 'Nature and Mathematics', restricts his historical range even more severely, and gives a lucid account of Galileo's approach, contrasted with the traditions of Plato and Aristotle. A. Rupert Hall's 'The Experimental Way' is really philosophy done by example: experiment may precede theory, or it may follow; it necessarily involves abstraction from raw nature, but may be misleading because of this. The examples range through the entire century, and here one feels that Hall has achieved relative clarity at the price of some oversimplification. The matter is not simply one of reasonable alternatives; natural philosophers of the time disagreed with each other, and struggled with their own ideas, in an attempt to find the best approach in any given context. The Descartes-Harvey controversy on the motion of the heart turned on this,[1] and would serve as a useful reminder of the difficulty of extracting Nature's secrets.

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[1] Letter to *American Scientist*, 45 (September 1957), 266A–274A, answering criticisms of his review of Needham's *Science and Civilisation in China*, Vol. II.

With Marie Boas Hall and Sam Lilley we are at the heart of the problem. ' The Machinery of Nature ' and ' The Nature of the Physical World ' discuss, from different aspects, the astonishingly sudden rise and acceptance of the ' mechanical ' view of the world. Lilley is most interested in the extrapolation of this view into the human sciences in the eighteenth century, but he is also concerned with the problem of its origin. This, he feels, cannot be satisfactorily explained by the internal dialectic of late sixteenth-century science; for an external cause he suggests, as a first approximation, the world-view of a commercial society. His problem with this is, of course, chronology: why should it be that the centuries-long decay of the ' organic ' Medieval society should suddenly catch the imagination of learned men in the first third of the seventeenth century? Marie Boas Hall sets herself the most difficult task of all: to describe the origins *and* outcome of the revolution in thought, and also to indicate its problems at various stages. Small wonder, then, that she has to fall back on sweeping generalisations to get her story told. Thus we find that ' the rational tendency of thought of the age . . . eagerly welcomed the mechanical view as the antithesis of the magical occultism of preceding ages.' Here is a causal explanation more closely tied to chronology than that of Lilley, but it still lacks precision. One is anxious to know more of this ' rational tendency ', and more of the ' magical occultism ' which was rejected: there was a world of difference between Fludd and della Porta; and the explicitly stated battle of the new philosophy was not with magic, but with the Aristotelian tradition.

Finally, Canon Raven tells his colleagues that they are barking up the wrong tree. Not astronomy but gastronomy is the fundamental human science, and the biological sciences were the prime movers of the revolution. Moreover, ' the heliocentric cosmology was less disturbing than the rejection of spontaneous generation, of creation as an act rather than as a process, and of witchcraft, astrology and magic.' It could be argued that the latter changes were the result of the former; but although the ' closed world to infinite universe ' change is commonly taken as the fundamental one, this priority has not, to my knowledge, been established by historical studies.

Thus this book shows us the history of science at an exciting stage in its development: it is sufficiently far advanced in the creation of a discipline, with a base in agreed fact, to venture into higher-order problems, and discussion of causes. Philosophers, as well as social and cultural historians, will have a contribution to make, for none of this generation of historians lacks respect for philosophy of science as an independent and important discipline. The only danger is that philosophers may misread surveys such as this book, and think that ‘little Jack Horner’ techniques will advance the history of science; but Gerd Buchdahl has shown that this need not happen, and that the two disciplines can be fruitfully combined. To sum up, this collection of essays serves admirably as a high-level introduction to the history of science at one of its most interesting and rapidly developing points.

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[1] J. A. Passmore, ' William Harvey and the Philosophy of Science ' *Australasian Journal of Philosophy*, 1958, **36**, 85–94. Etienne Gilson, *Études sur le rôle de la pensée médiévale dans la formation du système cartésien*, Paris 1930.

J. Ravetz

The Correspondence of Isaac Newton. Vol. I: 1661-1675; Vol. II: 1676-1687.
Edited by H. W. Turnbull.
Cambridge University Press (for the Royal Society). Vol. I: 1959. Pp. xxxviii + 468. Vol. II: 1960. Pp. xiii + 552. 7 guineas each volume.

Newton with his prism and silent face,
The marble index of a mind forever
Voyaging through strange seas of thought alone . . .

The familiar lines of Wordsworth on the statue in the antechapel of Trinity College, Cambridge, come to mind with emphasis on the last word, when one reflects that more than two centuries after Newton's death there is still no sign of a collected edition of his works and that there are masses of his manuscripts which have never yet been seriously and systematically studied by competent scholars. From time to time plans have been made, but the sheer magnitude of the task has daunted all concerned. Some years ago it was decided that the most that could be done ‘within the next decade’ would be to produce a complete and critically annotated edition of Newton's letters. In 1938 Professor H. C. Plummer agreed to undertake the editorship. After his death in 1946, the Newton Letters Committee was reconstructed under the chairmanship of Professor E. N. da C. Andrade, and Professor H. W. Turnbull was appointed editor. Recently, owing to ill-health, Professor Turnbull resigned (and has since died) and the work that remains to be done is in the hands of Dr J. F. Scott.

For the first time we can now get a general idea of Newton's correspondence. Of the 156 letters and documents in volume I, 137 have been published before but not together. Much of this early correspondence was with

Oldenburg and Collins, both of whom often acted on Newton's behalf in his relations with other scientists. Oldenburg was Secretary of the Royal Society until 1677 and kept in touch with scientists on the Continent. Collins was a civil servant in London who maintained a lively correspondence on problems of mathematics. With but two exceptions, all the letters in volume I deal with scientific or mathematical subjects. The first is a short note reproving a friend who was suffering from a hang-over. Its interest lies in the fact that it was written in a form of phonetic spelling of Newton's own devising. The other non-scientific letter, dating from 1669, addressed to Aston, later a Secretary of the Royal Society, gives advice on how to behave and what to observe when travelling abroad. For example, Aston is urged to ask questions rather than make peremptory assertions, ' it being ye design of Travellers to learne not teach '.

The two main subjects of the correspondence in Volume I are optics and mathematics and there is a remarkable contrast between Newton's willingness to impart his views on the former and his reluctance to do so in the case of the latter. Indeed, in one of the most interesting letters in this volume (No. 22), from Collins to James Gregory (in 1670), Collins, describing the only two occasions on which he met Newton and the problems which they discussed, writes with reference to an annuity problem, ' I wrote to him about the same, hoping to have had his Series communicated, with the method of its Invention, but I received no more in answer than what I sent you, hence observing a warinesse in him to impart, or at least an unwillingness to be at the paines of so doing, I desist, and doe not trouble him any more.' But for Newton's secretiveness, the regrettable priority controversy concerning the invention of the calculus need never have arisen.

The correspondence concerning optics deals with two main themes, the reflecting telescope and the composite nature of light. That relating to the latter is of supreme interest because it reveals the vital differences of outlook between Newton and his critics. For example, Huygens (letter no. 92) wrote to Oldenburg about Newton's theory of colours, ' il resteroit la grande difficultè d'expliquer par la physique mechanique en quoi conciste cette diversitè de couleurs'. In vain Newton replied to Oldenburg (letter no. 103), ' I never intended to show wherein consists the nature and difference of colours, but onely to show that *de facto* they are originall and immutable qualities of the rays which exhibit them, and leave it to others to explicate by Mechanicall Hypotheses the nature and difference of those qualities; wch I take to be no very difficult matter.' The clash was one of two different philosophies of science, just as was the later clash over the mechanism of gravitation.

In volume II many of the letters continue to be devoted to mathematics. Outstanding among them is the famous ' Epistola Prior ' of 13 June 1676, written by Newton to Oldenburg for transmission to Leibniz. It contains the first mention of the binomial theorem by Newton in his correspondence. Although he gives no proof of the theorem, he discusses several particular applications. Leibniz's comments are given in a memorandum (no. 171). In a further letter by him to Oldenburg (no. 172), dated 17 August 1676, he writes: ' Newton's discoveries are worthy of his genius. . . . His method of obtaining the roots of equations and the areas of figures by means of infinite series is quite different from mine, so that one may wonder at the diversity of paths by which one can reach the same conclusion.' This letter is indeed one of the most intriguing in the whole correspondence. For, Leibniz refers to his method as ' a corollary of a general theory of transformations ' which contains ' the true method of indivisibles as most generally conceived ', and the editor remarks that a full elucidation of Leibniz's account of his method in this letter would take us far towards a solution of the Newton-Leibniz controversy. The most general expression of the method of indivisibles, in the sense intended by Leibniz, occurs in the *Lectiones Geometricae* (1670) of Newton's predecessor in the Lucasian chair, Isaac Barrow, and it is an open question to what extent in his discovery of the calculus Leibniz was indebted to Barrow.

The ' Epistola Posterior ' of 24 October 1676 (Letter no. 188) is also of special interest because in it Newton reveals the steps which led to his early mathematical discoveries. In particular, he seems to have been greatly influenced by the arithmetical works of John Wallis.

In the latter part of volume II we first encounter Newton's ideas on motion and gravitation. In a letter to Boyle dated 28 February 1678/9 (no. 233), he puts forward his hypothesis of a universal aether ' much like air in all respects, but far more subtle ', and discusses it at length. At the end of this letter there is a paragraph in which he sets down ' one conjecture more which came into my mind as I was writing this letter. It is about ye cause of gravity.' He supposes aether to consist of parts of different degrees of subtility and that in the pores of bodies there is much less grosser aether in proportion to finer, than in the case of air and ' open spaces '. The grosser aether being less apt to be lodged in those pores than the finer aether below, it will endeavour to get out and give way to the finer aether below, and this can happen only if the bodies in question descend to make room above them for the grosser aether to escape into from them.

The most momentous correspondence in this volume begins with a letter (no. 235) from Hooke to Newton, written on 24 November 1679. In his capacity as Secretary of the Royal Society (in succession to Oldenburg) Hooke invited Newton to communicate a paper to the Society and mentions various recent discoveries in astronomy which had been made in Paris. In his reply of 28 November (no. 236) Newton communicates a suggestion for demonstrating the diurnal rotation of the Earth. Although the subsequent correspondence with Hooke soon exacerbated Newton, it had momentous consequences, for in a letter to Halley of 14 July 1686 (no. 290) he revealed that Hooke's letters led him to investigate central orbits with the result that during the winter 1679/80 he first proved that under the inverse square law the orbit could be an ellipse.

Much of the later correspondence in volume II is with Flamsteed and Halley, and relates in one way and another to the writing and production of the *Principia*. But there are also letters which bear on Newton's many other interests and activities. For example, he gave detailed advice concerning the special mathematical foundation established by Royal Charter in 1673 as part of Christ's Hospital to train forty King's Scholars in the art of navigation.

The superb production of these absorbing volumes reflects the greatest credit on all concerned, including the famous Press that has printed them, and particularly on Professor Turnbull whose notes are a most valuable aid to our understanding of the points at issue. Indeed, he draws our attention to many that might otherwise be missed. Both volumes have been fully indexed, but it would have been an added advantage if the tables of contents had contained under each item a brief reference to the main topics discussed in it, instead of merely stating the writer, the addressee, and the date.

G. J. Whitrow

Reason and Chance in Scientific Discovery. By R. Taton.
Hutchinson, London, 1957. Pp. 171. 30s.

Most British and American writers on the History and Methodology of Science have merely described when the discoveries in their sciences were made or have concentrated upon the logic of discovery. They have been interested in the formal connection between the discoveries they have chronicled, either temporally, or philosophically as within one world system or another. In either case they have regarded the act of discovery itself as outside their province. The logic of deduction and induction is discussed in philosophical terms, and lip service is paid to the psychological aspects of the processes involved.

Dr Taton refreshingly introduces his readers to the psychological problems involved in the act of discovery. His work has been well translated by A. J. Pomerans. He describes, with copious examples, some of the factors involved in discovery—flashes of thought, the rôle of chance and the rôle of error. He also describes what is involved in an original discovery and how some discoveries have been missed by great scientists working so close to the discovery that future generations have been forced to wonder why it was missed. The examples chosen are from many different fields and form a useful starting point for thought. One is struck by the number of continental references to writers also interested in this aspect of the philosophy of science. It would be of considerable importance to have other works in this field made more easily available for English speaking readers.

How successful is Taton's account? I think its success and its defect lies in its superficiality. Reading his account one can arrive at certain morals for the would-be scientist. One pictures the perseverance of the Curies, and can see that perseverance is, morally, a good thing in a scientist. Or one can see that without a broad theoretical grasp of a scientific situation one is going to be unable to see the relevance of a particular observation. There are many such moral precepts embedded in this book. However, this does not add up to the psychology of genius or the psychology of invention. We need to know much more about the psychology of the people described before we can be convinced of the real steps by which they associatedpreviously unassociated phenomena. We need to know more about the psychology of explanation and the motivations for it in a particular scientist's case before we can more than scratch at the surface of scientific theorising. One gets the impression that Dr Taton believes that the unconscious behaves as though its logic is that of the conscious. We have known since Freud that this is not so, and it is precisely in this field of unconscious associations that flashes of thought have their psychological coherence. Nowhere in the book is there a discussion of the obsessionalism that makes one worker wash his mistakes down the sink and that makes another notice a bactericidal fungus. Fleming is discussed, but not as a person.

Dr Taton is to be congratulated in making a step in the direction of the psychology of discovery. However, one wonders whether his goal can ever be achieved by writing history, for it is impossible to build up a science of the psychology of discovery by reference to untestable historical data. Much more needs to be done in the present before we can assess the correctness of his historical psychological theorising.

R. F. J. Withers

Variational Principles in Dynamics and Quantum Theory. By Wolfgang Yourgrau and Stanley Mandelstam.
Sir Isaac Pitman and Sons, London, 1960. Pp. xi + 180. 32s. 6d.

If someone intended to follow the history of physical theory as a search for harmony, continuity, economy, conservation, and 'reason' in general, he could hardly find a more lively exposition than Yourgrau and Mandelstam's *Variational Principles* whose second edition of 1960 is augmented by a timely chapter on quantum field theory. Ample quotations from the sources, far more revealing than the usual short notes quoted from one book to another, are combined with an extensive presentation of the mathematical material. Beginning with the discovery of ' nature's parsimony ' by Hero and Fermat in optics, and by Maupertuis in mechanics, we learn of the increasing recognition during the eighteenth century that minimum theorems are not to be taken as signs of reasonableness in nature's plan but as mathematically convenient transformations of mechanical laws, established before in differential form: If one has a differential equation, one can always find some quantity which is conserved, and some other quantity which under certain restrictions becomes an extremum, without *a priori* telling which quantity ought to have the prominent quality in question. Following the authors in their presentation of natural philosophy from Pythagoras to Aristotle, then *via* Ockham, Bacon, and Kepler to the mechanics of Newton, and to the integral principles of Maupertuis, Euler, Lagrange, Hamilton, and Jacobi, one is impressed by the increasing elegance and generality of the variational method as a powerful tool for the solution of intricate classical (as well as quantum) problems, testifying to ' the continuous transformation of a metaphysical canon to an exact natural law '.

Of particular interest is the mechanisation of the Maxwell-Lorentz field equations, which by their division into oscillators become adaptable to the Hamiltonian method and thereby also to quantisation. The untraditional mathematical method of Feynman, which rests on an integral condition rather than on a differential wave equation, is presented in readable form, to be recommended to any student of this difficult subject. Although it is not a variational method, Feynman's approach generates one under semi-classical conditions—analogous to Fermat's minimum principle which holds for optics under similar restrictions. It is a particular merit of the authors to have shown that Schwinger's method, though independently arrived at, is derivable from, hence equivalent to, Feynman's integral principle. These intricate mathematical developments are reduced by the authors to a minimum of formal complexity and a maximum of lucidity. The broad historical perspective of the book written in brilliant style, not forgetting the human angle, turns a tedious chapter of mathematical physics into a delightful reading matter also for the not-so-mathematical student interested in the development of mechanics from its stumbling beginnings to modern quantum mechanics of fields.

ALFRED LANDÉ

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

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Rankin, K. W., *Choice and Chance*, Basil Blackwell, Oxford, 1961, pp. 182, 18s.

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Schoek, H. and Wiggins, J. W. (Eds.), *Scientism and Values*, Van Nostrand Co., London, 1961, pp. xvi+270, 49s.

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Simpson, G. G., *The Meaning of Evolution*, Oxford University Press, London, 1961, pp. xv+364, 11s. 6d.
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Whyte, L. L., *The Unconscious before Freud*, Basic Books, New York, 1960, pp. xiii+219, $4.50
Witt-Hansen, J., *Exposition and Critique of the Conceptions of Eddington concerning the Philosophy of Physical Science*, Gads Forlag, Copenhagen, 1958, pp. 125
Witt-Hansen, J., *Historical Materialism: the Method, the Theories*, I, Munksgaard, Copenhagen, 1961, pp. 153, D.Kr. 58
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ANNOUNCEMENT

The 1961-62 Programme of the Boston Colloquium for the Philosophy of Science

The Boston Colloquium for the Philosophy of Science was initiated in 1960 as an inter-university interdisciplinary faculty group to foster creative and regular exchange of research and opinion, to provide a forum for professional discussion of papers and significant work-in-progress in the philosophy of science, and to stimulate the development of academic programmes in the philosophy of science in the Colleges and Universities in the Boston area. Members of the Colloquium are natural and social scientists, philosophers, and mathematicians. Four meetings, held in the spring of 1961, heard and discussed papers by C. G. Hempel ('Inductive Inconsistencies'), R. S. Peters ('Emotion and the Category of Passivity'), M. Bunge ('The Myth of Simplicity'), I. Scheffler ('Explanation of Psychological and Historical Events').

The programme of the Colloquium for 1961-62 includes the following meetings, to be held in the Boston area:

1961

26 October: 'Minds and Machines'
Norbert Wiener, Mathematics, M.I.T.
S. Watanabe, I.B.M. Research Center, Yorktown Heights, N.Y.
30 November: 'Language and the Formation of Concepts'
E. H. Lenneberg, Harvard University
Commentator: Roger Brown, M.I.T.
14 December: 'On the Conceptual Structure of Physics'
Laszo Tisza, Physics, M.I.T.

1962

8 February: 'Modal Logics I: Foundations'
Ruth Barcan Marcus, Philosophy, Roosevelt University
Commentator: W. V. Quine, Philosophy, Harvard University
1 March: 'Modal Logics II: The Formal Analysis of Cultural Objects'
Alan Ross Anderson, Philosophy, Yale University
O. K. Moore, Sociology, Yale University
22 March: 'Interpretations of Quantum Theory'
Ralph Schiller, Theoretical Physics, Stevens Institute of Technology
Armand Siegel, Theoretical Physics, Stevens Institute of Technology
Commentator: Abner Shimony, Theoretical Physics, Boston University and M.I.T.
26 April: 'The Falsifiability of Theories: Total or Partial? A Contemporary Evaluation of Duhem's Hypothesis'
Adoph Grünbaum, Philosophy of Science, Pittsburgh University
Commentator: R. S. Cohen, Physics, Boston University
10 May: 'Perception: Language and Perceptual Schemes'
Noam Chomsky, Linguistics, M.I.T.
Sidney Morgenbesser, Philosophy, Columbia University

For further information on the Colloquium, write to Professor M. W. Wartofsky, c/o Department of Philosophy, Boston University, Boston 15, Massachusetts.